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American SOCIOLOGICAL Review



Measurement in Sociology

Samuel A. Stouffer

Presidential Advice to Younger Sociologists

Past Presidents

The Quest for Universals in Sociological Research

Ralph H. Turner

Toward an Operational Definition of the Term "Operation" *Hornell Hart*

Some Comments on the General Theory of Action

Talcott Parsons

Juvenile Repeaters from Two Viewpoints

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Courtship Values in a Youth Sample *Eleanor Smith and J. H. G. Monane*

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A Comparative Study of the Role of Values in Social Action in Two
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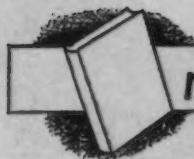
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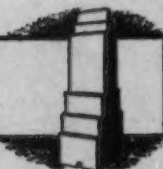
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American SOCIOLOGICAL Review

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Volume 18
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Official Journal of the American Sociological Society

MEASUREMENT IN SOCIOLOGY *

SAMUEL A. STOUFFER

Harvard University

WE now possess considerable knowledge about the conditions favoring inventions in Western culture. With particular reference to technology, sociologists like Ogburn and Gilfillan years ago cleared paths for better understanding. In Gilfillan's *Sociology of Invention*, for example, there are listed and documented some 39 propositions, or principles, relating to the appearance and acceptance of new ideas in technology. The last chapter of Conant's little book *On Understanding Science* is called "Certain Principles of the Tactics and Strategy of Science" and summarizes 21 propositions which he has illustrated from case studies of crucial developments in the history of physics and chemistry.

Why should not we, as sociologists, take an explicit look at the process of invention in the discipline of sociology itself, as a special case of the general working of invention in technology and science? This might not be easy, because all of us have vested interests in sociology which can bias us. But if students of culture do not examine their own discipline as a specimen of culture, who else will do it better?

My observations will be confined to only a limited segment of such undertaking, namely to some aspects of the place of measurement in the process of invention in sociology. I shall take the word measurement broadly to include the use not only of a metric, but also of ordinal position and even

of mere enumeration. Loose and inclusive as this is, my observations will be illustrative only, will omit important aspects of measurement in sociology, and will leave out entirely broad realms of description and analysis which are richly productive for sociology though not involving measurement. Such material would need the most careful consideration in any comprehensive treatment. As Cooley liked to point out, the phenomena of life are often better distinguished by pattern than by quantity. Hence, he suggested that a motion picture of the nesting habits of a mallard duck might tell far more about the duck than measurements of the tail feathers.¹ Even if I must leave to others the consideration of areas of investigation represented by many kinds of non-quantitative description and analysis which are staples of sociological, ethnological, and psychological literature, I wish no misunderstandings about my profound respect for their importance, especially in the exploratory phases of research, although I would not accord quite the same order of finality to the conclusions of, say, a DeTocqueville or a Sumner or a Freud, as Redfield seems to do in his challenging discussion of "The Art of Social Science."²

We have come quite a way in the last generation or two in the development of quantitative methods. We are even able to measure interactions, and to some extent behavior patterns, not just pin feathers. In-

* The Presidential Address read before the annual meeting of the American Sociological Society, held in Berkeley, California, August 30-31 and September 1, 1953.

¹ C. H. Cooley, *Sociology and Social Research*, p. 315.

² *American Journal of Sociology* (November, 1948), pp. 181-190.

deed, the advances in techniques have seemed so rapid as compared with advances in sociological knowledge that some scholars, in their less thoughtful moments, may yearn wistfully for a moratorium on technical progress to give our substantive knowledge a chance to catch up. The phrase "He's a mere technician" is a not uncommon epithet.

A central proposition in the theory of inventions is the postulate that an invention in technology or science ordinarily is not a discovery like an uncharted island emerging from the Pacific mist before the eyes of a Captain Cook, but rather is a long process of juxtaposing, in new combinations, complexes of elements all or most of which are already well known. Among the obvious conditions for such a new juxtaposition are a readiness to see it if it happens and the technical possibilities of seeing it.

The readiness to see it may be due to expectation engendered and disciplined by prior theory, or may be due simply to some combination of habits of curiosity, habits of sharp observation, and luck. The technical possibility of seeing it may depend on the prior existence of an entire technology or a combination of science and technology such as lies behind an electron microscope or a modern super-calculating machine.

Let us get to work by examining, for illustrative purposes, a sociological topic in which there is a current interest—the subject of role, and role expectations or obligations. Suppose we would like to study some such proposition as: If a person has obligations in roles X and Y which conflict when situation S occurs, the probability is high that he will sacrifice X and continue in Y. Being scientists, at least by aspiration and self-designation, we want to be the authors not only of an idea but of an idea which can be shown to be wrong if it is wrong or right if it is right.

In the first place, we may need to show what a given role obligation is. If we consider a range of behavior extending from what would never be tolerated to what would be applauded as behavior beyond the line of duty, we will probably visualize a range within that range which would not be subject to severe disapproval at the one end and would not transcend the bounds of favorable expectation at the other. Within that nar-

rower range may lie a still narrower range of behavior which might represent the range of normal expectation, though this might not coincide with or sometimes might not even overlap a range of ideal expected behavior. Moreover, it is likely that such a nest of ranges will not be perceived the same by all members of our social group. There will be variability in individuals' perceptions. We have a measurement problem on our hands.

We look into our carpenter's chest of measuring techniques and find a good many tools. These tools have a history. Without the hundreds of man-years which have gone into them we could hardly get started. We are dealing with attitudes toward certain kinds of behavior. The direct measurement of attitudes is rather new, but indirect measurement based usually on inferences from official collections of data—such as Durkheim used in *Suicide*—are somewhat older. Behind the invention of attitude measuring devices are many complexes of inventions. There is the cumulated experience of testing intelligence and various aptitudes. There are two or three generations of laboratory work in psychophysical measurement. There is a variegated experience in fields like market research and public opinion research—illustrating vividly Conant's observation that science often owes as much to applied and commercialized technologies as they in turn owe to science; lessons learned by practitioners in question-wording, interviewing, and sampling are a case in point. There is a large and growing body of mathematical theory and practical computing methods to provide statistical tests of measurement adequacy. And there are several competing techniques, some of which seem to be mutually contradictory. We have seen in the past generation numerous examples in our own field of what Ogburn and Dorothy Thomas called parallel inventions, only a small fraction of which are likely to survive in anything like present form. Thus associated with such names as Thurstone and Guttman are quite different models for approaching ostensibly the same goals. Out of efforts to reconcile seemingly disparate ideas often come new and better ideas. And we must not overlook the contribution of hardware. Some kinds of measurements or analysis—factor analysis is an example—

would be prohibitively costly if not impossible except for modern computing machines. The latest electrical computers are opening up new regions in statistical theory, stimulating lines of inquiry which otherwise might never have been started.

Frustrations growing out of inadequacies in direct measurement stimulate search for alternatives. Most of the conventional techniques for measuring attitudes assume that a respondent can or will answer a direct question about his attitude. But Freud and those who preceded and followed him produced evidence to the contrary. It is easy to demonstrate discrepancies. Getzels, for example, was interested in studying the norms in a Northern women's college with respect to association with Negroes.³ The official tradition being liberal, almost all the girls in a dormitory said that they personally would not mind having a Negro as a roommate, but when asked how their friends in the same dormitory felt, they tended to report that their friends would dislike the idea. Either the girls misperceived or misrepresented their friends' attitudes or misperceived or misrepresented their own. Such a result is obviously relevant to our desire to measure role expectations properly. A possible way out may be the use of what are called projective techniques. Here again is a complex, or several competing complexes, of inventions. Techniques like the Rorschach ink-blot test or the TAT picture interpretation test, or various forms of the Sentence Completion Test, which have a history of indebtedness to a number of streams of influence, are still controversial. Most of the inventors in this field have been clinical psychologists or psychiatrists. The consequence is illustrative of propositions in the theory of inventions. Being clinicians, the inventors often had little interest in psychometrics or concern with whether their tests met acceptable statistical requirements of reliability or validity. Hence enthusiastic hopes for the efficacy of such tests tend to be cooled when objective standards are used to check clinical intuitions concerning their efficacy. If, on the one hand, the imaginative inventors were

limited by lack of contact with modern statistics, on the other hand the conventional psychometricians, who were in contact neither with psychiatric theory nor with clinical cases, were in no position to dream up such tests in the first place. Only if a combination of the two can be achieved are inventions likely to result which will have both the needed rigor and the needed imagination.

In addition to measurements which might derive from respondents' reports, there is the possibility of measurement which might be derived from an investigator's own observations of verbal or non-verbal behavior of the persons concerned. The Hawthorne studies of role behavior in an electrical plant were able to measure the range in individual output tolerated by members of a small work group which set its own restrictions on production even though members might at least in the short run profit more by higher productivity.⁴ Such observations often are difficult to make and costly if an observer has to be on the spot more or less continually for a long time. Moreover, actual behavior as observed may bear little or no relation at times to ideal behavior, which still may have to be ascertained from questioning respondents. Techniques for systematic recording of on-going behavior are themselves a complex of many ideas. Time-sampling procedures for observing children in play groups were introduced at Minnesota a generation ago by Olson, Goodenough, Anderson, and others, and developed further by Dorothy Thomas and associates at Yale. Gadgets like Chappel's interaction recorder were elaborated by Bales and others into devices for classifying interaction in small groups into any of several categories at the same time as the activity is observed. (It may be noted parenthetically that the practical use of such a device requires much skill if two observers are to record the same thing simultaneously and thus guarantee reliability. Whether it is Ogburn or Gilfillan speaking, or Conant on the strategy and tactics of science, we hear that the longest and hardest part of the job in producing an innovation which will

³ J. W. Getzels, *The Assessment of Personality and Prejudice by the Method of Paired Direct and Projective Questionnaire*, Harvard Ph.D. dissertation, 1951.

⁴ Roethlisberger and Dixon, *Management and the Worker*, Cambridge: Harvard University Press, 1939.

take hold is usually the job of making it practical and economical. Much of the effort of Bales and his associates has gone into the study and development of techniques for economical training of accurate observers. He even has constructed a special machine with vari-colored lights whose sole purpose is to cut down training time). The new Minnesota laboratory for small-group studies has built-in electrical gadgets for obtaining subjective reports via push buttons which the subjects operate at the same time as their interactions are also recorded by outside observers. Such a device is, like most inventions, only a slight alteration of older devices, many of which in this case also involved the transfer of button pushes to a record on a continuous sheet of paper.

Since in the sociological illustration we are carrying along with us we are interested in measuring role expectations, we must not overlook another technique which, in combination with others previously mentioned, might be useful to us. I refer to role playing. This technique, which Moreno and those trained by him did so much to develop and elaborate, has significance for us far beyond eliciting projective-type information from an individual subject who is taking an unaccustomed role. For it opens a new range of possibilities in the experimental study of group behavior where our experimental subjects are, we hope, "behaving naturally" and where others in the group are trained role players serving as stooges who can shift the course of action according to a pre-arranged design. The technical problems involved in the selection, training, and utilization of stooges are many and perhaps still only dimly appreciated. Much the largest part of the time, for example, involved in an experiment recently reported by Mills⁵ went into learning how to make the role players behave. Some day this will probably become a sub-discipline of its own, with canonical treatises on *The Care and Feeding of the Stooge*. Role playing is, of course, not a technique of measurement, but it is a method which provides ways of checking the possible validity of devices like pencil and paper tests, as well as opening the way to

otherwise impossible experimental designs where measurements can be made.

Well, it is evident that if we wish to measure role obligations we have quite a body of technical knowledge available. We are heirs to the efforts of countless people—of whom, incidentally, only a handful were sociologists and of whom many were not even academicians. Yet with all this wealth of know-how, we will find the measurement process an anything but easy task, because none of our tools have the precision or the fool-proof character that we want. This does not mean, of course, that we need to succumb to counsels of despair.⁶

Let us now suppose, as a prelude to our remaining observations, that we can measure what we are after.

Even if we can measure the competing obligations in roles X and Y respectively, we would fall short of our initial objective unless we show that, given conflict in situation S, there is a high probability that a person will sacrifice X and continue in Y. This implies the design of an experiment which contains the possibility of some kind of quantitative test, even if crude. Or, if not an experiment, a plausible facsimile which of course would mean depressing our initial sights.

The demand for experimental proof is rather young in Western culture, as Conant emphasizes in his exposition of physics and chemistry. It is still younger in fields which are closer to sociology or social psychology and are better examples for us in many respects, like biology or medicine. Except for a few dramatic developments in anatomy and physiology, the habit of experimentation in medicine is only a few decades old and hardly has begun in psychiatry. Conant repeatedly emphasizes the tremendous difficulties involved in pioneering experiments where the problem now looks simple and obvious, yet where failure to identify significant variables and control them vitiated results and even retarded progress for decades. So much more complicated was the problem of controlling variables in biology that decisive results in some areas

⁵ Theodore M. Mills, "Power Relations in Three-Person Groups," *American Sociological Review*, 18 (August, 1953), pp. 351-357.

⁶ Such as sometimes seem to be preached by writers like Herbert Blumer. See Blumer's incisive "What is Wrong with Social Theory," unpublished paper presented at the current session of the society.

had to wait on new techniques of experimental design based on mathematical statistics. The statistical innovations of Fisher, which in turn stood on the shoulders of many predecessors—including, importantly, an applied statistician in the Guinness brewery—and have been elaborated by hundreds of followers, are possibly among the necessary conditions for successful experimentation in fields like ours, where the cost of a single experiment in time and money is so great that many of the less relevant but still disturbing variables may have to float around loose and be gathered up in a statistical net rather than be separately controlled. The literature on the mathematics of experimental design and on its applications is now increasing at such an exponential rate that non-specialists cannot keep up with it. Although the mathematics becomes more esoteric and difficult, some of the results are in the direction of facilitation of simpler designs at lower costs with minimal loss of information. Ideas of sequential analysis advanced by Wald and as yet little developed in practice may cut experimental costs in two or better. Use of order statistics instead of statistics based on a metric may give surprisingly good approximations to far more exacting and expensive procedures and permit the use of simple measurements not previously admissible. The making of such ideas practical is, as might be expected from invention theory, proving to be much more time consuming than the initial formulation of the ideas. And there are areas important to sociological experimentation where we have as yet no technical help. For example, there is yet no appropriate probability model with which to formulate a null hypothesis for testing the significance of measures based on a time sequence of interactions within an experimental social group.

The frequent unreadiness of mathematical statistics to facilitate our experimental designs is only one of the handicaps which is holding back experimentation in sociology. Perhaps even greater is the lack of accumulated and transmissible experience in practical arts of handling people in the design and execution of an experiment. Possibly the major proportion of the time spent in training a chemistry Ph.D. is used not in expounding known formulas but in teaching

him the arts of the laboratory—transmitting myriads of small cues and skills, including such humble ones as when to suspect that a test tube is about to blow up in his face. Only a few sociologists are now trying to do experiments—and these in not more than a half dozen or so institutions. If, however, as few as ten new Ph.D.'s in sociology a year get training in experimentation and continue to practice and train others who will in turn train others, the curve of transmissible experience will soar, just as it has in the past thirty years in the practical use of statistics. There will still be barriers of cost and it remains to be seen how much we really mean what we say when we salaam before the ideal of verification. For proof comes high. And it is by no means always rewarding to the experimenter if it throws cold water on cherished ideas. Having sponsored a limited amount of such experimentation in the Armed Forces during the last war and in the Laboratory with which I am now associated, I have some rather painful memories.

Sociology perhaps never can look forward to the relatively cheap kind of experimentation which grows a thousand different molds simultaneously, hoping to find in one of the thousand jars a new antibiotic. Because of sheer cost, each experiment will have to be preceded by much thinking, to satisfy as well as possible in advance two questions: (1) If the experiment verifies my hypothesis, so what? (2) Can the experiment be done? The answer to the first question calls for a framework of larger theory, even if that theory is a modestly light and shaky scaffolding capable of being blown down by the first gust of serendipity. The answer to the second question calls for a choice of empirical data such that the numerical values obtained by manipulating the variables will not be obfuscated by even larger errors of measurement. We must search hard for our tobacco mosaic molecule which, though not necessarily intrinsically important, has relatively easily observable properties which can be generalized further. One of Conant's main observations from the early history of natural science is of the failures of experimentation because of choice of materials which could not be accurately enough measured on the instruments of the day.

There is another function of controlled

experimentation in sociology which has not received the attention it deserves. That is, to provide an ideal design against which the imperfections of less adequate designs reveal themselves as a caution against over-confident interpretation. We owe much to Chapin for his studies of imperfect facsimiles of experimental design. If we cannot intervene ourselves and introduce the stimulus situation which forces an individual to choose between conflicting role obligations, perhaps we can observe persons whom we infer to be in the throes of such a choice. Bales, for example, has seen in problem-solving small groups that the best initiator of ideas seemingly cannot also remain the best liked member of the group over a sequence of sessions and that he tends to sacrifice his role of instrumental actor rather than sacrifice his role of good friend.⁷ Merton has seen in an interracial housing project a trend toward what he calls value homophily, and has observed the strains when a pair of families who are friends feel they either must yield their friendship or else alter their conflicting values with respect to race relations. Lazarsfeld has translated Merton's problem into a simple model based on a 16-fold table, which has the property of specifying what numerical values to look for, given shifts of pairs of variables in two points in time. He also has generalized further to n points in time, showing that certain initial conditions and tendencies to yield in conflict should result more rapidly in an equilibrium than others.⁸

Unless we can control crucial variables, however, mere changes in time between two variables or mere correlation between two variables in a given point in time may be highly deceptive substitutes for changes which we as experimenters might induce on initially matched groups. We who toiled on the researches in *The American Soldier*, only a few of which satisfy the experimental ideal, are keenly sensitized to this problem and, I fear, drove not only ourselves but also some of our readers to distraction in our efforts to control enough variables to get within hailing distance of the ideal even when we

could not overtake it. Re-examining some of this material, Lazarsfeld and Kendall performed an office of formalization in the book *Continuities in Social Research*, which should be the precursor of a growing series of logical analyses of approximations to experimentation.⁹

Finally, even if we establish the particular "if then" hypothesis as to role conflict which we have used illustratively in this paper, there will be a most compelling temptation for overgeneralization beyond this one study. There may be no effective immunization against this, but a better knowledge of how inventive processes work may help build up resistance. Let us remember that no one feat, however heralded, is seldom very important in the totality of feats by hundreds, possibly thousands, of different people which usually are necessary before an important development in either science or technology is firmly established. Nothing is more deceptive for example, than to tag the electric light with the single name of Edison. The following question may be more sobering in the future than it has been in the past, when the significance and finality of particular sociological contributions were too hopefully applauded: How can sociologists preserve their humility without losing their enthusiasm?

In my remarks today I have left unsaid much which should have been said. More is needed, for example, about the direct impact on theory of research design involving measurement, by forcing theory to become operational. Problems involved in the invention and development of useful mathematical models have been passed over. Fortunately, on this same program Dodd, one of our most zealous pioneers, has provided examples of such models which are not mere sketches for future testing but which already have been subjected to test. But I do hope that the illustrations and suggestions which I have advanced will encourage others to examine the subject of measurement in sociology more extensively and systematically, as a specimen of the inventive process in Western culture.

Conant's Principle Number 9 reads, "A

⁷ Parsons, Bales and Shils, *Working Papers in the Theory of Action*, Glencoe, Illinois: The Free Press, 1953, Chapter 4.

⁸ Merton and Lazarsfeld, "The Dynamics of Value Homophily," unpublished manuscript.

⁹ A recent and searching general analysis of the problem appears in an unpublished study by Donald T. Campbell entitled "Designs for Social Science Experiments."

scientific discovery must fit the times." Gillfillan's Principle Number 36 reads, "An invention coming before its time remains undeveloped and practically useless." Are the times now ripe for making sociology cumulative by advancing "if then" ideas which can be proved wrong if they are wrong or right if they are right? I think that many, if not all, of the necessary ingredients are now present in our sociological culture. These ingredients are highly complex collections of ideas, of recorded experience, and of research techniques, some of them mathematical.

Who will put these ingredients together in sociology? Not the philosopher, speculating in his arm chair. Not the sensitive artist, watching human activity with a dramatist's

eye. Not the statistician who is solely concerned with making a better probability model or measuring device. Rather, the sociologist who combines several of these skills in his own head, or the small sociological team which brings a few specialists together in a concerted enterprise. Then theory will beget research and research will beget theory, and the Malthusian upswing of sociology will be on its way, slowly—oh, so slowly at first and so painfully—but on its way, with acceleration. To students in our colleges and universities who may hear these remarks or read them: on behalf of those who have permitted me to be spokesman for the American Sociological Society in 1953, may I bid you welcome into a brave new world.

PRESIDENTIAL ADVICE TO YOUNGER SOCIOLOGISTS

(EDITOR'S NOTE: The following consists of the recorded program of the luncheon meeting in Berkeley, August 30, 1953. At the invitation of President Stouffer, each living ex-president of the Society recorded a brief message. These were assembled on a single record, which was played for the first time at the above session. The program is unaltered, except that the brief introduction of each speaker by Dr. Stouffer is omitted.)

The records may be obtained from the Executive Office of the Society, New York University, Washington Square, New York 3, New York. For the single record (2 side) standard long-play 33 RPM records, with the forty-five minute (approximately) program, the price is two dollars and fifty cents.)

SAMUEL A. STOFFER: Fellow sociologists, here in California today at the 1953 annual meeting of the American Sociological Society, we are about to hear the recorded voice of each of the living former presidents of our Society. As the present incumbent, I am happy to introduce my predecessors. Each will speak for about two minutes on the following topic: What are the best words of counsel you can give to a young Ph.D. just launching his or her sociological career?

JOHN L. GILLIN (*University of Wisconsin*): Gentlemen: I do not commiserate you that you begin your careers in sociology at a time in which perplexity and confusion plague men and nations and when fear possesses so many. No, I congratulate you. A review of the periods of history when there was an outburst of great thinkers on social relationships indicates without doubt that these were periods of vast confusion, of fundamental changes which upset the established organization of society and which brought doubt and frustration to many individuals. These conditions presented a challenge. The thinkers answered that challenge with great courage, with formulated diagnoses, and made suggestions as to how the society could be organized to convert the disorganization which challenged them into an organization fitted to the needs of men. We are in the midst of such another period, perhaps in some respects the most complex of any in history. Some of us who are speaking to you today have lived through rapidly changing conditions which have challenged us to think our ways through the maze. We have tried but there is so much yet to be done. Sociology is yet so young, so immature. So much remains to fill even some of the

larger gaps in our knowledge. So often we are guessing, speculating where we have no facts. Our theories are often born of our prejudices or our wishes. Before *you* is the opportunity and the challenge to make sociology increasingly scientific and so contribute more effectively to the readjustment of a disordered world.

WILLIAM FIELDING OGBURN (*University of Chicago*): Sociologists operate in three zones, teaching, research and public service. You should consider the apportionment of your time between the three and which shall have priority when a choice must be made, as for instance between science and social action, on the basis of either time or principle. Science is the pivot around which all methods turn. Therefore, think out your conception of science. A monastic vow to science means, though, not being tempted away by the indulgence of emotional bias, by the sweet applause of public service, or by the fun of intellectual activity without the work of verification. Avoid being a sociological hermit by being scientific only part of your time; always in laboratory and seminar, often in publication, when necessary in classroom, occasionally on a public commission, but never in the drawing room. In playing these different sociological roles, you should not deceive others nor fool yourself by wearing the garments of science falsely or inappropriately. Cultivate multiple sociological personalities rather than a single integrated one. Finally, success will come to those with good working habits, with the drive of ambition and with a sense of the significant, assuming, of course, that your I.Q. is OK.

HOWARD W. ODUM (*University of North Carolina*): For my firstly, I would recite Pavlov's requisites for the young scientist, namely gradualness, modesty, passion. And I would add maturity, creative ability with originality, intellectual integrity, and singleness of purpose as stepping stones to greatness. For my secondly, I would urge disciplinary morale, by which is meant not only knowing sociology but believing in it as the most dynamic of the social sciences, and as having its greatest opportunity and obligation up to now. For my thirdly, I would urge that the subject field of sociology must always connote human behavior as an energy and mankind as the crucial societal-constant

in society, even as the natural environment and biological heritage are relatively constant in natural science. This means, in the Conant dictum, a sharp cleavage between animal behavior and human conduct and, in the Cooley dictum, make it total and make it human. For my fourthly, I urge that we mature the ability to equate in perspective the data of historical and descriptive sociology with those of controlled observations in concrete situations. And finally, I would urge the discovery of fundamental assumptions about the data and methods of studying society that will be useful in contemporary society in which the traditional social order is in conflict with the expanding technological order.

EMORY S. BOGARDUS (*University of Southern California*): Before giving counsel in the dark, as it were, to young Ph.D.'s in sociology I have written to thirty of them asking that they formulate any problems on which they would welcome counsel. The sample is not representative but varied and the respondents in all seriousness described a total of 54 problems, some of which were given by several respondents. Only ten of these problems will be cited here. (1) What are the best ways to go about getting a position in sociology? (2) Should I seek a position in a small college, a university, in government research or in research in a private industrial or social welfare organization? (3) What is a wise division of time between teaching, doing research, counseling students, performing community activities, and assuming family responsibilities? (4) Are there any objective methods for determining how effective I am as a sociology teacher? (5) What aims should one have in teaching the introductory course in sociology? (6) Should a young sociologist give any thought to social philosophy? (7) Can one have both freedom and security in teaching sociology today? (8) Upon what bases is the direction that sociological research is taking today decided? (9) How can a young sociologist best prepare for economic security in his older years? (10) How important is it that one attend the meetings of the American Sociological Society? Having exceeded the 200 word limit, I must leave the answers to these questions to my older and wiser colleagues.

ERNEST W. BURGESS (*University of Chicago*): Sociology has a unique role among the sciences of human behavior. It is at the one and the same time a humanistic and a natural science. It is humanistic in the sense that its data are distinctively the attitudes and values of persons, groups and societies. Accordingly, the interview is the research technique particularly adapted to obtain data on the sociological aspects of social phenomena. Sociology as a natural science seeks to establish propositions about human behavior that are universal wherever people live in association. A second and less ambitious objective is to secure generalizations that are limited to a given culture or subculture. As a generalizing science, sociology has two main research methods. One of these, statistics, has in recent years had a tremendous development. At present it overshadows the other method, that of case study. This imbalance greatly hampers the progress of sociology and should be corrected. A prerequisite to this end is the recognition of the significance of the personal document for the understanding of personal and collective behavior. Its value can hardly be overestimated. It is to the sociologist, what the microscope is to the biologist. It makes visible and feasible for research what cannot be perceived in observable behavior. It provides in recorded form data on attitudes and values for analysis by the case study method. The perfection of interview techniques, the further development of case study methods and their integration with statistical techniques are a challenge to the enthusiasm, the energy and the ingenuity of the oncoming generation of sociologists.

F. STUART CHAPIN (*University of Minnesota*): You should seek to become known as a social scientist in the field of your specialty. Do not be misled by resistance to overspecialization. True integrations which fill interstitial areas between older specialties, themselves become new specialties. Produce scientific studies in your own specialty, but do not be afraid to write a textbook which integrates areas of specialization. Remember that research, scholarship and teaching are interacting behaviors, each one reinforces the other. Techniques are no more effective in the long run than the soundness of the basic logic that underlies them. Technique should

not become an end in itself lest you remain a skilled artisan instead of a creative worker who invents and discovers. Technique should not become a snobbish escape device in which you seek mere intellectual security. Do not fear intuitive insights which may lead to fruitful new hypotheses. Remember that social values are an important subject matter of study and value judgments contribute to decisions on what to study. But try to keep your value systems from undermining the objectivity of your research method. Do not mix your roles, since as a leader of an action program you cannot simultaneously remain an objective observer of this same action program.

HENRY PRATT FAIRCHILD (*New York University*): In setting out to acquire a Ph.D. the chances are that you were looking forward to teaching as a career. If so, it was because you regarded teaching as an honorable, socially valuable, personally rewarding profession. Now that you have achieved your degree, one of your basic motivations should be to keep teaching on the high level that you envisioned it. This will not be easy. There will be many temptations and opportunities to relax your standards in the interests of recognition, preferment or even pecuniary compensation. Just now, in this year 1953, there are especially powerful inducements to sacrifice your ideals on the altar of personal tranquility and security. Expediency presses you to trim your sails to the winds of orthodoxy, conformity and subservience. The trail of your profession lies in that quarter. My counsel to you is to cherish and maintain your scientific integrity at all costs, and the cost may be high. The price of individual independence and social accountability may be your job, your livelihood, possibly even your life. But you will preserve that greatest of all human values, a decent respect for, and a high opinion of, yourself.

ELLSWORTH FARIS (*University of Chicago*): By this time you know that sociology is not a matured science, and that makes its pursuit difficult. But you confidently expect it to be perfected, and that makes it exciting. Future Sumners and Cooleys will arise, and whence but from your own ranks? A basic science of human nature is essential to our survival. Who will create it but you? But you must be scientific, for we need new

demonstrable knowledge. Unless your work be characterized by disinterestedness, rationality and objectivity, it is not science and will not endure. Be not misled by the Swedish doctrine that prejudice is unavoidable, nor by the exhortations of those who want you to reform the world. Leave crusades and agitation to others. Be scientific. When you discover new knowledge, publish it. But forbear to insult the English language. Some there are who write so as to doom the reader to ignorance of what the writer is thinking or whether he is thinking at all. Be not like unto them. Learn to write good English. But if nature has not endowed you with creative originality, be content to remain a useful, self respecting mediocrity, teaching what you have been taught, forbearing to spoil white paper with worthless publications. Although some may heed these words, others will ignore them. They will learn the hard way.

FRANK H. HANKINS (*Smith College*): On this occasion one wishes he were a humorist. Far better to be briefly amusing than to seem to future generations a pretentious ass or an ignorant fool. However, uncertainty seems excusable because sociology still is uncertain of its proper *métier*. It still lacks an accepted general theory serving as basis for solid accumulation. Yet we have learned much in 50 years. We now seek understanding rather than reform or uplift. Statistical competence has become a professional necessity even though mountainous labors sometimes produce mousy conclusions. We have learned that statistical findings are tarred with an inherent relativity to time and setting, that prediction is precarious and that social telesis has been indefinitely postponed. The interrelations of culture and personality have been clarified but such studies cannot supply the basic theory needed for the study of cultural evolution. Culture is *sui generis* and not explicable in terms of human nature. The latter sets limits and shapes the contents but does not determine the course of cultural development. In this age of social revolutions, when civilizations seem perishing under our very eyes, it is time to renew and double our earlier efforts to understand the causes of such transformations. The basic problems thus become those of cultural change and trends of shifts in population and resources, in wealth, income, and social prestige, in

social structure and class alignment, and in the redistribution of social power and the concurrent shifts in the controlling system of values.

ROBERT M. MACIVER (*Columbia University*): Somewhat reluctantly, I join my colleagues in the easy business of offering difficult advice. I presume we all think advice is in order because the road a sociologist should follow is uncharted and contains many pitfalls not to speak of diverting side-tracks that lead nowhere in particular. For my part, I find a great contrast between the vast significance of our subject matter and the relative insignificance of what we do with it. Such advice as I can give is therefore confined to a single point. I do not know how or why you became a sociologist. I do not know why you have been willing to go through the somewhat tedious and often vexacious process of acquiring the Ph.D. Forget about all that. Your job now is through increasing knowledge and reflection to throw some more light on some part of your chosen field. Your field is the ways and the wildernesses of human relations. Don't study any more, don't spend any more time—I mean of course as a sociologist—except along lines where you have a reasonable hope that what you do is really contributing to the explanation and to the interpretation of human relations, alike in your role as a teacher and in your role as an investigator.

STUART A. QUEEN (*Washington University*): In our efforts to develop sociology as a science we have properly laid great emphasis on research but too often have overlooked the importance of teaching. I have heard more than one sociologist speak scornfully of teaching yet whence will come future researchers unless someone teaches them? Of what avail is all our research unless its results are passed on to others? And why should the public support us unless we deliver something deemed of value? Personally, I have found the problems and the rewards of teaching as great as those of research. Now if we are to teach effectively in the classroom, through the printed word, or otherwise, we will find it profitable to express ourselves clearly in English that will attract attention, gain respect and convey meaning. Too often

we mistake polysyllabic neologisms for wisdom, technical jargon for instruction, and the confused structure of sentences and paragraphs as evidence of superior intellect. Of course scientific terminology is necessary and so is English diction that is orderly and dignified. Young man or woman just endowed with a Ph.D., don't forget the importance of communication.

GEORGE A. LUNDBERG (*University of Washington*): My counsel is as follows. First get literate somehow as early as possible; learn to read and write. These are not mere literary attainments. They are part and parcel of rigorous mental behavior of the kind that gets results in science. Therefore get, as soon as possible, a thorough grounding in logic, mathematics and semantics. Second, don't waste your time on courses consisting of somebody's advice on current issues and problems. Most of these issues will be dead before you get out of graduate school. Better get hold of some techniques that can be used on any issue that may arise, especially those that will confront you when you leave school. In fact, don't waste too much time on courses and lectures. Get a job on a real research project under the direction of someone who knows the ropes. Don't think you can make a career out of the same chatter that some ex-president got away with. These may have been new and important ideas at one time. Times may have changed, we hope for the better. Get away from beaten paths unless they continue to yield pay dirt on their own merits. Finally, if you feel you already know the answers and merely want a pulpit from which to expound them, keep out of the profession of research and teaching in academic institutions. The future of these institutions depends on their ability to maintain detachment with respect to social program. Go in for special pleading when you feel like it but don't mistake your special pleading for science.

RUPERT B. VANCE (*University of North Carolina*): Has there ever been a more exciting period for a gifted young sociologist to step across the threshold. For the first time, the field of social theory is being developed with regard to actual research of attainable dimensions. Moreover, research methods are maturing apace in terms that

will take sociologists out into the field and turn up data that we never knew existed before. How shall the young sociologist orient himself in the midst of this embarrassment of riches. Let him appropriate unto himself a chosen sector in our developing framework of theory. Let him master several rewarding techniques and devote continuing study to the major problem of research design. In addition to the field of theory, why should not our young Ph.D. stake out an area to explore in depth. It can be another culture, his own metropolis, his region or a small community whose intertwined forces he would like to unravel. All this will help much more if he abandons the unwieldy jargon entombed in his dissertation and learns to write the English language with some pleasure as a medium of communication. The best British scholars write in this fashion and their work is a joy to read. Nothing will help one more to become a good teacher and that is a distinction I greatly crave for any promising young sociologist.

KIMBALL YOUNG (*Northwestern University*): First I would emphasize the importance of content in sociology. In recent years there has been a high stress on quantitative methods to the neglect of the substantive aspects of our field. Since the great bulk of our young Ph.D.'s will spend most of their lives as teachers rather than as high specialists in research, they need adequate instruction in content. This is not to disparage methodology but to restore a more healthy balance between method and substance. Secondly, we should continue our efforts to tie up our theoretical formulations to empirical research. Some of the writings of certain contemporary theorists are just short of verbal obscurantism which contribute little or nothing to research. Let the young Ph.D. avoid such theoretical pitfalls. Finally, as a member of the presidential gerontocracy, I enter a plea for tolerance for the older sociological publications. There is a disposition, in some quarters at least, to look askance at any book or article which was not produced day before yesterday. There is a wealth of sound stuff in the best of the older writing. In our anxiety to be up to date and to start afresh, let us not cast out solid ideas just because of their age.

CARL C. TAYLOR (*United States Department of Agriculture*): As a young Ph.D. you are presumably well trained in conceptual thinking and have already done an acceptable piece of research. You are now moving into professional activities where you and not someone else will call the signals on what you do and how you think. You can use borrowed concepts and preach them to every student that you teach, believing that thereby you lengthen the shadows of the revered men who taught or disciplined you. Or you can use their concepts and concepts of others, your own included, to help thread your thinking through bodies of significant social phenomena about which some of your revered instructors knew very little. If you do research it will be for more significant purposes than gaining a Ph.D. degree but it will have to meet more ruthless and more realistic tests than passing a Ph.D. examination. It will, however, have some possibility of being useful. Most of you will be compelled to give a major portion of your time to teaching great masses of students, most of whom have no intention of being sociologists. Teach them something that will help them to be intelligent citizens and you will be a successful sociologist.

E. FRANKLIN FRAZIER (*Howard University*): To devote oneself to the study of sociology indicates first a faith in the possibility of the application of scientific methods to the study of human behavior. This faith should not lead, however, to the naive assumption that man will automatically accept the findings of social science in preference to superstition and false belief. There is no large market for sociological knowledge in order to build a more rational world. Men will utilize sociological knowledge because they want to pursue certain interests or attain certain ends. Nevertheless, the sociologist should stick to his path and not be turned aside because of political considerations or even expediency. All this implies that the sociologist should be sophisticated as well as intellectually honest. Next, the young sociologist should be sure that when he undertakes research he defines his problem within a sociological frame of reference. It has become fashionable to turn sociological problems into psychological problems, which is always a much safer and more popular

procedure. Finally, the young sociologist should seek answers to significant questions concerning man in society and not strive simply for perfection in the use of techniques. Techniques cannot be substituted for thinking and in sociology, as in other sciences, advances can be made only through reflection and constructive thought.

TALCOTT PARSONS (*Harvard University*): Gear yourself to the fact that sociology is new and rapidly developing. Try to balance solidity with adventurous intellectual daring. Dilettantism and superficial brilliance won't win you a place in intellectual history but neither will stodgy playing safe when great ideas are stirring. Be solidly grounded in your general field and its history but don't be a colorless eclectic who has neither special competence nor ideas of his own. Specialization within limits is an indispensable basis of solid accomplishment and a natural result of creative enthusiasm and no man can do everything well. But don't take the conventional boundaries of your specialty for granted. They are always provisional and your own creative work may change them. Every sociologist should be trained and actively interested in theory but only a minority can or should be specialists in theory. These few however may play a vital role in the future of sociology. Creative theoretical thinking is perhaps the highest achievement in any field of science and certainly without it no amount of empirical industry or ingenuity will attain our goals. If you feel deeply interested and are sure of your talents don't let yourself be scared away from theory as a specialty. Sociology, particularly in America, is going to need your contribution.

LEONARD S. COTTRELL (*Russell Sage Foundation*): During the past 25 years, the minimal requirements for competence in sociology have become both more rigorous and more generally agreed upon. The trends have markedly accelerated since 1945 and during your career they will carry sociology to a mature professional status. In order to plan the acquisition of the essentials for growing with your profession, you will do well to appraise your training and experience to date as to the extent to which they have provided you with the following: (1) A

thorough grounding in both qualitative and quantitative research methodology and research design. This includes sufficient competence in mathematics to deal creatively with the problems of measurement and analysis. (2) Sophistication as to the function of theory in research and as to the present status of theorizing in sociology. This includes a grasp of the problem involved in a shift from monadic to interactional theory and from a passive descriptive to an active experimental orientation. (3) Substantial experience in collecting and analyzing empirical data. (4) Intimate acquaintance with a substantive field of knowledge and with some of the problems of practical life situations to which this knowledge refers. (5) A knowledge of the history of science. (6) A working knowledge of the sister disciplines most relevant to your field of interest. (7) The capacity to use your theoretical and methodological tools without allowing your scientific imagination to be limited to their limitations. (8) The imagination to identify strategic scientific problems and the courage to work on them however remote from practical life situations they may seem. The odds are very good that you will find your training, as I have found mine, deficient in some of these respects. But the extent to which you can plan to meet these criteria is a reasonably good indication of your prospects of becoming a competent and even an outstanding sociologist.

ROBERT COOLEY ANGELL (*University of Michigan*): Young sociologists will do well to study the history of their discipline and learn its lesson. Members of the first American generation were tempted into over-broad theorizing by their concern to analyze the socially significant. Because of the generality of their statements their theories have rarely been subject to the kind of test that makes a science truly cumulative. The second generation, resolving above all to be rigorously scientific, swung over to the consideration of data about which precise statements could be made and often ended in triviality. You sociologists of today and tomorrow must somehow strike between. You must bring really significant areas of fact under the order of scientific law. This will require that rarest of qualities, scientific imagination. You cannot each hope to be a Newton, a

Darwin or an Einstein, but many of you can cultivate something of the qualities that made them great. The main thing is to brood over important sets of facts, looking at them first in one perspective, then in another, firmly resolved to find the simplifying theory that brings order out of chaos. Disciplined thought playing over carefully collected data is the royal road to scientific advance. No amount of methodological sophistication, and that is important too, can take the place of fertile imagination firmly anchored in rigorous theory and in extensive knowledge of the empirical world.

DOROTHY S. THOMAS (*University of Pennsylvania*): The most recent ex-president quotes from her presidential address an appraisal of her experiences in interdisciplinary research. I have not found it profitable to separate economics from the behavioral disciplines or to neglect in behavior situation studies the realities of economic structure, economic differentials and economic development. I have not found it profitable to merge disciplines at the "conceptual" level. It is the data of economics, rather than the elaborate systems of the theorists, that have provided a basis for practicable procedures. I have not found it profitable to proceed as if all behavior must be or even can be "transmuted" into quantitative terms. I still push the statistical aspect to the limit but no longer relegate the subjective and the descriptive to secondary positions. I have belatedly recognized that we are all theorists and all statisticians, that in our research underlying theory must be made explicit, and hidden statistical generalizations squarely faced. I have found it profitable to take "disciplinary" leaves of absence from interdisciplinary research to fill in gaps in training and technique. For more than three decades, I have found interdisciplinary research a rewarding and integrating experience. Ending the quotation, this ex-president therefore counsels the young Ph.D. just launching his or her career, to take interdisciplinary research rather than to leave it alone.

SAMUEL A. STOFFER: To our past presidents, may I say thank you. Modesty made some a bit reluctant at first to preserve their spoken words, as one expressed, like a fly in amber. The counsels offered are varied, some-

times are contradictory. That's good. For all will hope that nothing said today to our young Ph.D.'s will inhibit a daring willingness to pick up torches which others found too hot to handle or which in older hands merely sputtered and went out. And all

through this unique symposium there glows a faith in the future for those who will build the sociology of the last half of the twentieth century. This program has been made possible by the facilities of the Columbia Broadcasting System.

THE QUEST FOR UNIVERSALS IN SOCIOLOGICAL RESEARCH *

RALPH H. TURNER

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IN a book which has maintained attention and perhaps increased in influence over two decades, Florian Znaniecki describes the method he names "analytic induction," and designates it as *the* method which should be adopted in all sociological research.¹ Analytic induction is merely a special name for one formulation of a basic philosophy that research must be directed toward generalizations of *universal* rather than *frequent* applicability.² But Znaniecki's statement is unusually unequivocal and is specifically oriented toward sociological research. Hence it makes an excellent point of departure for a study of contrasting methodologies.

Znaniecki's position has recently been challenged by W. S. Robinson, who depicts analytic induction as an imperfect form of the method Znaniecki calls enumerative induction.³ Robinson's contentions are further discussed by Alfred Lindesmith and S.

Kirson Weinberg in replies to his paper.⁴ The three discussions extend our understanding of the method, but leave some questions unanswered.

Methodological advance requires more than the mere tolerance of alternative methods. Any *particular* methodology must be examined and assessed in the light of the total process of research and theory formulation.⁵ Accordingly, the objective of the present paper is to offer a definition of the place of the search for universals in the total methodology for dealing with non-experimental data. The procedure will be to examine specific examples of empirical research employing the analytic induction (or similar) method, to note what they do and do not accomplish, to establish logically the reasons for their distinctive accomplishments and limitations, and on these grounds to designate the specific utility of the method in relation to probability methods.

EMPIRICAL PREDICTION

Robinson's contention that actual studies employing the method of universals do not

* This paper has benefited from discussion with W. S. Robinson's seminar in methodology and from a critical reading by Donald R. Cressey.

¹ Florian Znaniecki, *The Method of Sociology*, New York: Farrar and Rinehart, 1934.

² This point is brought out by Alfred R. Lindesmith in his comments in the *American Sociological Review*, 17 (August, 1952), p. 492.

³ W. S. Robinson, "The Logical Structure of Analytic Induction," *American Sociological Review*, 16 (December, 1951), pp. 812-18. Robinson's argument may not altogether escape a logical pitfall. He first makes a careful description of the analytic induction procedure, but does it by describing its elements within the framework of statistical method. Any such operation necessarily slights any aspects of the first framework which lack counterparts in the second. The conclusion that analytic induction is a special but imperfect

form of statistical procedure would then be inherent in the operation itself rather than a legitimate finding.

⁴ "Two Comments on W. S. Robinson's 'The Logical Structure of Analytic Induction,'" *American Sociological Review*, 17 (August, 1952), pp. 492-95.

⁵ Lindesmith's statement that, "Statistical questions call for statistical answers and causal questions call for answers within the framework of the logic of causal analysis" (*ibid.*, p. 492), seems to be an evasion of the problems of *why* and *when* each type of question should be asked. "Methodological parallelism" is of dubious fruitfulness.

afford a basis for empirical prediction appears sound. However, it is only when the method is made to stand by itself that this limitation necessarily applies. Furthermore, the reason for the limitation is more intimately linked to the intrinsic logic of the method than the incidental fact that investigators using the method have tended to neglect the right-hand side of the four-fold table.⁶ These statements may be substantiated and elaborated by an examination of selected studies.

Lindesmith's well-known study of opiate addiction will serve as a useful first case. The causal complex which is essential to the process of addiction involves several elements. The individual must use the drug, he must experience withdrawal distress, he must identify these symptoms or recognize what they are, he must recognize that more of the drug will relieve the symptoms, and he must take the drug and experience relief.⁷

From the standpoint of predicting whether any given individual will become an addict or not, the formulation has certain limitations. First, it does not tell who will take the drug in the first place, nor give any indication of the relative likelihood of different persons taking the drug.⁸ Second, the thesis itself affords no cue to variability in intensity of withdrawal symptoms, nor any guide to instances in which the symptoms will be mild enough not to result in addiction. Third, the theory does not provide a basis for anticipating who will recognize the symptoms and the means of securing relief. Fourth, personal and social factors involved in taking or not taking the drug to relieve the identified distress are not indicated. We cannot predict in an empirical instance unless there is some way of anticipating which people, given exposure to the drug, will recognize

the nature of the withdrawal symptoms, will identify the means of relief, and will take that means of relief.⁹ Finally, Lindesmith's theory does not indicate to us what will be the pattern of the addict's behavior, since this is determined by the cultural definition and treatment of the drug and its addicts. In sum, Lindesmith provides us with a causal complex which is empirically verified *in retrospect*, but which does not in itself permit prediction that a specific person will become an addict nor that a specific situation will produce addiction.

Donald R. Cressey's statement regarding the violation of financial trust likewise is posited as a system of universal generalizations and is similar to Lindesmith's in format.¹⁰ Three elements are essential to trust-violation. The person who will violate a financial trust has, first, a "non-sharable financial problem," a difficulty which he feels he cannot communicate to others. Second, he recognizes embezzlement as a way of meeting this problem. And third, he rationalizes the prospective embezzlement, justifying it to himself in some way.

First, the points at which Lindesmith's and Cressey's statement are parallel and points at which they are not parallel may be noted. The withdrawal symptoms and the non-sharable problem can be equated as the conditions which require some relief which cannot be secured through conventional channels. There is also a parallel between recognition that the drug will relieve the distress and recognition of embezzlement as a possible solution to the non-sharable problem. On the other hand, because drug addiction ensues from but one type of problem,

⁹ Lindesmith does not overlook these considerations in his descriptive treatment of the process. However, his treatment of them remains anecdotal and impressionistic rather than systematic and they are not integrated into the rigorous statement of his theory. The nearest he comes to a systematic statement concerning one of these variables is his observation that, "as long as a patient believes he is using the drug solely to relieve pain, and regards it as a 'medicine,' he does not become an addict." (*Ibid.*, p. 56). Weinberg suggests the use of measurement in some of these connections (*op. cit.*, p. 493).

¹⁰ *Other People's Money*, Glencoe: The Free Press, 1953. A brief statement of the theory also appears as, "Criminal Violation of Financial Trust," *American Sociological Review*, 15 (December, 1950), pp. 738-43.

⁶ W. S. Robinson, *op. cit.*, pp. 814-16. The writer doubts that this limitation inheres logically in the conception of analytic induction as described by Znaniecki.

⁷ Alfred R. Lindesmith, *Opiate Addiction*, Bloomington: Principia Press, 1947, pp. 67-89, *et passim*.

⁸ Some of Lindesmith's argument with current theories of drug addiction (*ibid.*, pp. 141-64) rest upon a difference of purpose. Some of the theories he criticizes can be defended if reworded in terms of likelihood of first taking the drug in other than a medical treatment situation, rather than in terms of the likelihood of becoming addicted.

withdrawal distress, Lindesmith can specify the taking of an opiate as essential. Cressey can specify no specific "first step" because of the variety of problems which may come to be non-sharable. The rationalization stage is absent from Lindesmith's formulation though he discusses it as a *frequent* phenomenon.

It is difficult to find a logical reason why rationalization should be *essential* in the one instance and merely *frequent* in the other. Perhaps the explanation lies, not in the logic of the phenomena themselves, but in the conditions necessary for a sense of closure on the part of the investigators. Since Lindesmith is explaining the existence of a continuing psychological state, it is sufficient for his purposes that the prospective addict be carried from a particular state of recognition (the symptoms and role of the drug) to an overt act with specific psychological consequences (relief by taking the drug). Cressey, however, is explaining a single action and so he seeks to fill the gap more fully between the particular state of recognition (that embezzlement will solve a non-sharable problem) and the act of embezzling, which he does with the rationalization.¹¹

In light of the parallels between the two schemes, it is not surprising that the same limitations with regard to empirical prediction apply to Cressey's statement as did to Lindesmith's. The theory does not indicate who will have non-sharable problems, what specific conditions will make a problem non-sharable and in what circumstances a problem may cease to be non-sharable. Nor do we have a guide to the circumstances surrounding recognition of embezzlement as a solution to the problem. And, finally, there are no systematic indicators of who will be able to rationalize and who will not.

There are perhaps two general reasons why the Lindesmith and Cressey studies do

not produce empirical prediction, reasons which are applicable because of the very specifications of their method itself. One of these reasons has already been extensively illustrated, namely, that there is no basis for determining beforehand whether the conditions specified as necessary will exist in a particular instance.

The second general reason for lack of empirical prediction is that the alleged *pre-conditions* or essential causes of the phenomenon under examination cannot be fully specified apart from observation of the condition they are supposed to produce. In any situation in which variable "A" is said to cause variable "B," "A" is of no value as a predictor of "B" unless we establish the existence of "A" apart from the observation of "B." This limitation is in particular applicable to Cressey's study. Is it possible, for example, to assert that a problem is non-sharable *until* a person embezzles to get around it? If a man has not revealed his problem to others today, can we say that he will not share it tomorrow? The *operational* definition of a non-sharable problem is one that has not been shared up to the time of the embezzlement. Similarly, Cressey must be referring to some *quality* in the recognition of embezzlement as a solution which may not be identifiable apart from the fact that under appropriate conditions it eventuates in embezzlement. With embezzlement techniques and tales of successful embezzlement a standard part of the folklore of banks, offices handling public and private payrolls, and the like, mere recognition of embezzlement as a solution to problems is probably a near-universal characteristic of persons in a position to be able to embezzle. Similarly, rationalizations of embezzlement are part of the folklore and their use is standard joking behavior among persons in such positions. Consequently both recognition of embezzlement as a potential solution and ability to rationalize the act only become discriminating conditions when some sort of qualitative or quantitative limitation is imposed upon them. But under the present formulation it is only possible to identify what is a sufficient recognition or a sufficient ability to rationalize by the fact that they eventuate in embezzlement.

Lindesmith's theory, though less subject to this limitation, reveals the same vulner-

¹¹ Perhaps there is an object lesson indicated by this comparison. If the perspective of the investigator can determine what will be necessary for inclusion as the *essential* elements, there may be no theoretical limit to the number of such perspectives and consequently to the variations in what is considered essential. Such an observation would make Znaniecki's dictum that the investigator can arrive at a point beyond which no new knowledge about a class can be added difficult to defend. (Cf. Znaniecki, *op. cit.*, p. 249).

ability. Since withdrawal distress varies in degree according to size of dose and the number of shots taken, and since several shots may precede the existence of addiction as Lindesmith defines it, definition of the point at which the individual is taking the drug *to relieve withdrawal distress* as distinct from the point at which he is simply taking another shot must be arbitrary in some cases. But the distinction is crucial to Lindesmith's theory, since before this point the individual is not addicted and presumably may interrupt the process, while after this point he is addicted and the process is complete. Hence, the identification of what constitutes an effective recognition of the relief the drug will bring can only ultimately be determined by the fact that addiction follows such recognition.¹²

As a final case, we shall refer to a study which is in important respects rather different, but which is couched in terms of a parallel logic. In Robert C. Angell's well-known study of fifty families that suffered a serious reduction in income during the Depression, he attempted to work out a set of categories which could be applied to a family before the Depression which would predict how it would respond to the drop in family income. On the basis of assessments of "integration" and "adaptability," Angell "predicts" the response to financial crisis in terms of a "vulnerability-invulnerability" continuum and a "firm-readjustive-yielding" continuum.¹³ Through his designation of a presumably comprehensive pair of concepts for describing those characteristics of the family which are essential in predicting his post-crisis variables, Angell follows an analytic induction model, though his variables are not simple attributes as are those of Lindesmith and Cressey.

On the surface, Angell's formulation looks a good deal more like a device for empirical prediction since he provides categories which can be assessed before the process of responding to the Depression gets under way

and without reference to the consequences. A careful examination of the nature and manner of assessment of the two essential variables will indicate whether the impression is justified.

The idea of integration seems to refer to the degree to which a family is a unit, which is a fact not observable in the same direct sense as the fact of taking a drug, for example. Integration conveys a meaning or feeling which is recognized by a number of symptoms, such as affection, common interests, and sense of economic interdependence. Integration in practice, then, is identified by an impressionistic assessment of several observable variables.¹⁴ Of these variables there is no single one by which alone integration can be identified, nor is there any single "symptom" which may not be lacking in families classified as highly integrated.

The prediction which is provided by this scheme is *theoretical* prediction according to an analytic induction model. But the theoretical prediction cannot be converted into empirical prediction unless integration can be assessed beforehand. The assessment is made by an implicitly statistical operation, a mental weighting of several items of observation. In order, then, to gain *empirical* prediction the investigator shifts over to an "enumerative induction" procedure.

The concept of adaptability is both more important¹⁵ and more complex, combining two elements as Angell uses the term. First, if a family has been flexible in the face of minor crises or problems that have occurred in the past, it is said to be adaptable and the prediction that it will maintain its unity in the face of a larger crisis is consequently made. This, of course, is merely an application of the principle that there is a con-

¹² Lindesmith admits some vagueness on the matter of what genuinely constitutes knowledge that an opiate will relieve withdrawal distress, but regards the vagueness as a present limitation of his knowledge rather than an intrinsic limitation of his method. Cf. *op. cit.*, p. 77.

¹³ Robert C. Angell, *The Family Encounters the Depression*, New York: Scribner's Sons, 1936.

¹⁴ Not only is the weighting of the various data of observation impressionistic but these criteria are themselves impressionistic. The implicitly statistical nature of Angell's operation has been noted before and his documents subjected to a restudy under Social Science Research Council auspices. In the restudy, scales for the measurement of integration and adaptability were devised to objectify ratings and translate them into numerical values. Ruth Cavan, *The Restudy of the Documents Analyzed by Angell in "The Family Encounters the Depression."* Unpublished.

¹⁵ Reuben Hill, *Families Under Stress*, p. 132, citing Cavan, *op. cit.*

stancy in the response of a given system to situations of the same sort, and has no causal significance. The other aspect of adaptability consists of a number of criteria, such as commitment to material standards, concerning which the same comments apply as in the case of integration.

Thus in the three cases cited empirical prediction is not provided by statements of universally valid relationships taken alone. What, then, do such efforts accomplish?

ANALYTIC INDUCTION AS DEFINITION

What the method of universals most fundamentally does is to provide definitions. Not all definitions are of equal value for deriving scientific generalizations, and the definitions produced by the analytic induction procedure are intended to be characterized by causal homogeneity.

The effort at causal homogeneity is evident in the refinements of definition that accompany the method. In the process of attempting to generalize about addiction Lindesmith had to distinguish between those drugs that produce withdrawal distress and those that do not. Early in his work he concluded that it would be futile to seek a single theory to explain both types. Cressey points out that he could not study everyone who is legally defined as an embezzler. Unless he restricted his subjects, for example, to those who entered the situation in good faith, he could not form valid generalizations having universal applicability. Angell also rules out certain types of families. He recognized that some of his families were units merely in a formal sense, and that he could not observe uniform principles which would be applicable to the latter.

Saying that the principal accomplishment of the search for universals is to make definitions depends upon showing that the generalizations which it produces are deducible from the definitions. This is clearest in the case of Lindesmith's theory. In Lindesmith's presentation he has outlined the essential stages in becoming addicted by the time that he has arrived at his full definition of the phenomenon. The essential stages are im-

plicit in the concept of addiction as he presents it.¹⁶

In place of the empirical attributes viewed essential by Lindesmith, Angell constructs two theoretical categories to which he ascribes the character of essentiality. But Angell is really getting the definition of his causal variables from the dependent or effect variables which he sets up. Adaptability seems to correspond to the firm-yielding dimension and integration to the vulnerability dimension. Adaptability and integration are the logically deducible counterparts to the dependent variables.

Cressey's formulation is less completely amenable to this interpretation. The recognition of embezzlement as a solution is a logically deducible component, since one cannot perform a purposive self-conscious act unless its possibility is recognized. By definition the subjects of Cressey's study possessed long standing conceptions of themselves as law-abiding individuals, and were socially recognized as such at the time of the offense. While perhaps not from the definition alone, at least from the body of established theory which is implicit in the definition, it follows that the individuals must at the time of the crime in some way reconcile their behavior with their law-abiding self-conception. Indeed, we cannot help wondering whether failure to report rationalization could be entirely independent from the criteria by which an investigator would exclude some subjects from his study on grounds of doubting the honesty of their initial intentions.

The non-sharable problem, however, is probably only partially deducible. Given the fact that all people have problems that might be solved by stealing, given the fact that these subjects were mature individuals, and recognizing that they must, by definition, have resisted situations in the past which could have been improved by stealing, then it would seem to follow that a very distinctive type of problem would be required for people to deviate from their established life-patterns. The non-sharability of the

¹⁶ W. S. Robinson has suggested this in his "Rejoinder to Comments on 'The Logical Structure of Analytic Induction,'" *American Sociological Review*, 17 (August, 1952), p. 494.

problem might be deducible as a *frequent* characteristic, but probably not as a universal characteristic.

Thus, with the exception of non-sharability, the theories that have been examined serve chiefly to delimit a causally homogeneous category of phenomena, the so-called essential causes of the phenomenon being deducible from the definition.

It is, of course, not accidental but the crux of the method that these generalizations should be deducible. It is through the causal examination of the phenomenon that its delimitation is effected. The operation in practice is one which alternates back and forth between tentative cause and tentative definition, each modifying the other, so that in a sense closure is achieved when a complete and integral relation between the two is established. Once the generalizations become self-evident from the definition of the phenomenon being explained, the task is complete.

THE INTRUSIVE FACTOR

The next step in our argument must be to ask why the search for universals does not carry us beyond formulating a definition and indicating its logical corollaries, and why it fails to provide empirical prediction. The answer may be that there are no universal, uniform relations to be found except those which constitute logical corollaries of conceptual definition. The positing of operationally independent causal variables, empirically assessable prior to the existence of the postulated effect, always seems to result in relationships of statistical probability rather than absolute determination.¹⁷

A minor reason for these limited findings is the fact of multiple determination, with which analytic induction is rather ill-equipped to cope. When such complex phenomena as family integration, rather than individual behavior, are examined, the method very rapidly shifts into the ideal-

type technique, which is no longer subject to the sort of straight-forward empirical verification as analytic induction. As in Angell's study, the logic of the method is preserved but the empirical problems become quite different.

But as the central thesis of this paper we shall call attention to another explanation for the absence of universal, uniform relations which are not logical corollaries of definitions. The "closed system," which is the core of Znaniecki's statement and whose isolation is the objective and accomplishment of the method, is a causally self-contained system. As such, it is not capable of activation from within, but only by factors coming from outside the system. While, by definition, uniform relations exist within closed causal systems, uniform relations do not exist *between* any causal system and the external factors which impinge on it. *External variables operating upon any closed system do not have a uniform effect because they have to be assimilated to the receiving system in order to become effective as causes.* The outside variable has to be translated, in a sense, into a cause relevant to the receiving system. Normally there will be alternate ways in which the same external variable may be translated depending upon the full context within which it is operative. The situation in which a man finds himself, for example, can only activate the closed system of the embezzlement process when it becomes translated into a non-sharable problem. Cressey finds no type of problem, phenomenologically speaking, which necessarily and uniformly becomes a non-sharable problem.

The external factor which activates a system may be referred to as an *intrusive* factor. This idea is taken from Frederick Teggart's discussion of what he calls an "event." "We may then define an event as an intrusion from any wider circle into any circle or condition which may be the object of present interest."¹⁸ There are always intrusive factors which are accordingly not predictable in terms of the causal system

¹⁷ These remarks and some of the subsequent observations must be qualified by noting that Cressey's "non-sharable problem" is an apparent exception. If the statements in this paragraph are correct we should expect further research to eventuate either in some modification of the concept, "violation of financial trust," or in the re-evaluation of the non-sharable problem as a *frequent* rather than essential characteristic.

¹⁸ Frederick J. Teggart, *Theory of History*, New Haven: Yale University Press, 1925, p. 149. Quoted by Clarence Marsh Case, in "Leadership and Conjuncture: A Sociological Hypothesis," *Sociology and Social Research*, 17 (July, 1933), p. 513.

under examination, but which serve to activate certain aspects of the system. The same idea may be thought of as levels of phenomena. There are no uniform relations between levels of phenomena, only within levels.

Empirical prediction always concerns the way in which one closed system is activated by various intrusive factors. Hence empirical prediction always requires some statistical or probability statements, because there is some uncertainty or lack of uniformity in the way in which the intrusive factors will activate the causal system and even in *whether* they will activate the system.

UNIVERSALS AND STATISTICAL METHOD

The utility of defining universals within closed systems lies in the translation of *variables* into *concepts*. A variable is any category which can be measured or identified and correlated with something else. A concept is a variable which is part of a theoretical system, implying causal relations. That correlations among variables, of themselves, do not provide a basis for theory, or even for anticipating future correlations, is well known. Analytic induction fails to carry us beyond identifying a number of closed systems, and enumerative induction fails to go beyond the measurement of associations. The functions of the two methods are not only distinct; they are complementary. When the two methods are used together *in the right combination*,¹⁹ they produce the type of findings which satisfies the canons of scientific method.

What the identification of closed systems does is to provide a basis for organizing and interpreting observed statistical associations. For example, valid research would probably reveal some correlation between liking-to-run-around-with-women and embezzlement. Cressey's findings do not discredit such an observation but afford a basis for interpreting it. In the light of certain American mores

such a behavior pattern is likely, in some circumstances, to create a problem which would be difficult to discuss with others. The crucial aspect of this behavior for the determination of embezzlement would be its creation of a non-sharable problem.

With the closed system described it is possible to take the various correlations and get order from them. Identification of the closed system also gives us guides to significant variables, correlations that would be worthy of test. At the present point it should be profitable to search for the kinds of situations which most often become non-sharable problems, the characteristics which are correlated with the ability to rationalize an activity which would normally be regarded as contrary to the mores of society, the personal and situational characteristics associated with taking opiates (other than by medical administration) sufficiently to experience withdrawal symptoms. A study of correlations between certain sex patterns and the acquisition of non-sharable problems would build cumulatively in a way that a study of correlation between the former and embezzlement would not do. Some quantitative measure of such correlation would in turn provide the basis for using the closed system formulation for empirical prediction.²⁰

One useful indication of the way in which a statement of universals can function in the total research operation is afforded by Edwin Sutherland's "differential association" theory of criminality.²¹ While this theory is not the product of a specific empirical research operation of the sort that Lindesmith or Cressey undertook, the form of Sutherland's proposition is that of the analytic induction model. He employs a felicitous term in stating his theory. Differential association, he says, is "the specific causal process" in the genesis of systematic criminal behavior. He

²⁰ Cressey proposes a study of such related conditions in much the same manner as is indicated here, but does not clarify whether this should be by a further extension of the method he has used or by the measurement of probabilities. Cf. *Other People's Money*, Chap. V.

²¹ Edwin H. Sutherland, *Principles of Criminology*, Chicago: J. B. Lippincott Co., 1939, pp. 4-9. The third edition of Sutherland's work is cited here because he has modified the features of his theory most relevant to the argument of this paper in his fourth edition (1947).

¹⁹ In no sense can those research reports which devote a section to statistical findings and another section to case study findings be said to illustrate the thesis of this paper. In most cases such contrasting categories refer only to the method of data collection, the method of *analysis* being enumerative in both cases, but precise in the former and impressionistic in the latter.

does not say that differential association is *the* cause or the *only* one; poverty and the like may be in some sense causes. But differential association is the specific causal process through which these other factors, or more removed causes, must operate. Poverty and other correlated factors only facilitate criminal behavior because they affect the person's likelihood of learning a pattern of criminality from a model of criminality which is presented to him. The differential association theory identifies a hypothesized closed system, in terms of which the many correlated variables gain their meaning.

There are many theories already extant which have this same character, but which have not always been viewed as logical counterparts to the analytic induction method. Edwin Lemert's proposition that, "The onset of insanity coincides with the awareness of one's behavior as being individually different from that of all other people's," points to the same sort of *specific causal process* in the genesis of insanity, or "secondary psychotic deviation."²² And Sorokin's interpretation of Durkheim's theory of suicide follows the same form.²³

Statements of this sort are devices for placing in bold outline the meaningful components of the phenomenon under study. In order to achieve the form of a universally valid generalization the investigator either states his causes as inferential variables (Angell), or states empirically continuous variables as attributes (Lindesmith, Cressey). In the latter case, the dividing point between the two phases of the crucial attribute is identifiable only retrospectively on the basis that the specified sequence is or is not completed. But if the essential components of the causal complex are viewed as continuous variables, capable of measure-

ment independently of completion of the hypothesized sequence, the *essential degree* of the components will vary from instance to instance. Hence, in the process of designating the essential causes in a manner susceptible to empirical identification prior to their expected effect, the investigator must recast his thesis in terms of probability rather than uniform and universal relations.

A danger of the search for universals lies in the inadequate utilization of much valuable data. Cressey has information on the types of backgrounds his subjects came from, but because these are not universals the information has been filed away, or handled impressionistically. Lindesmith likewise secured abundant information which he uses only to demonstrate that absolute uniformity does not exist. Angell describes the frequent characteristics of the integrated and the adaptable family, but he does not systematize this material because such aspects of it are not universals. In these cases the imposition of particular methodological restrictions has limited what can be found out about the phenomenon under examination.

Analytic induction or some logical counterpart of the method is an essential aspect of research directed toward accumulating an ordered body of generalizations. But, for the reasons developed in this paper, Znaniecki's statement that, "analytic induction ends where enumerative induction begins; and if well conducted, leaves no real and soluble problems for the latter,"²⁴ represents an untenable position. It is through conceiving the "essential" conditions in a closed system as the avenues through which correlated factors can operate as causes, that generalizations about closed systems can escape their self-containment and probability associations may be organized into meaningful patterns.

²² Edwin M. Lemert, *Social Pathology*, New York: McGraw-Hill, 1951, p. 428.

²³ Pitirim A. Sorokin, *Society, Culture and Personality*, New York: Harper, 1947, pp. 8-13.

²⁴ Florian Znaniecki, *The Method of Sociology*, p. 250.

TOWARD AN OPERATIONAL DEFINITION OF THE TERM "OPERATION"

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STUART DODD said, in 1943: "A definition is operational . . . to the extent that the definer (a) specifies the procedure . . . for identifying or generating the definiendum and (b) finds high reliability . . . for the definition."¹

More than 140 articles relating to operationism have appeared in learned journals in recent decades. Yet there has been almost complete absence in these articles of any systematic attempt to define operationally (in the sense stated above by Dodd) the basic terms of operational theory. The present article is an attempt to apply such a process to definitions of the term *operation*.

PROCEDURES USED IN THE FIRST EXPERIMENT

Two separate experiments in this field have been carried out. In the first, the following four definitions were subjected to tests of reliability:

1. Webster's *Collegiate Dictionary* (1936), in addition to other definitions presented the following: "*Operation*: An action."

2. P. W. Bridgman said, in 1945, that (from the point of view of the utmost generality): "*Operation* is to be understood in the sense of any conscious activity."²

3. A definition which might be acceptable to radical operationists is as follows: "*An operation* is any action by a biological organism, performed by voluntary (striated) muscles."

4. A definition which might be acceptable to moderate operationists is as follows: "*An operation* is any physical or mental change (from that which otherwise would have occurred) which the operator produces intentionally, in himself or in his environment."

In order to test the reliability of these

definitions, judges were invited, in April, 1953, to apply each of these four definitions to each of 20 phenomena. Of these, 19 were identical with those numbered as follows in Table 4: 1, 3, 5 to 20, and 22. The one not listed in Table 4 was: "To know that one is moneyless."

As judges to apply the above definitions to these 20 phenomena, 80 graduate students at Duke University were invited to cooperate. The students invited consisted of the University fellows and graduate assistants in the following departments: Sociology, Political Science, English, History, Economics, Religion, Philosophy, and Physics. From 51 of these students, replies were received in time to be employed in the present analysis.

To score the unreliability of a given definition, as applied to a given phenomenon, the following method was used. First (by a process to be described in a later section of this article) the judges were divided into two groups, consisting of 25 "better" and 25 "poorer" judges. Two negative points were scored for each disagreement ("Yes" by one judge and "No" by the other) in each possible pair in each group of 25, for each of the 20 phenomena. One negative point was scored for each pair in which one judge responded "?" and the other either "Yes" or "No." The maximum negative score possible for a given definition of a given phenomenon would, therefore, be $2 \times 12 \times 13$, or -312. The unreliability score was then recorded as the percentage which actual disagreements constituted of the maximum number of possible disagreements. For example, among the 25 "better" judges, applying the Webster definition to the phenomenon "To shift one's attention involuntarily," 11 classified it as an operation, 10 said it was not an operation, and 4 entered a question mark. Twice 10 times 11, plus (4 times 21), equals 302, which is 97 per cent of the possible maximum.

¹ Stuart C. Dodd, "Operational Definitions Operationally Defined," *American Journal of Sociology*, 48 (1943) p. 482.

² Percy W. Bridgman, "Some General Principles of Operational Analysis," *Psychological Review*, 52 (1945) p. 246. He commented: "However, such great generality is of little use."

PROCEDURES USED IN THE SECOND
EXPERIMENT

Before presenting the results of the first experiment, a brief description of the second one will be given. A copy of a mimeographed preliminary report on the first experiment was sent to various people, including Professor Bridgman. He commented as follows, in a letter dated May 27, 1953:

"... As you yourself say, I have never given a formal definition of what the operation is. This has been deliberate on my part. I have always insisted that perfect sharpness is not attainable or necessary, and I think I have said somewhere that the best way of learning what the operational approach is, is to see it in action as applied to many concrete examples. At the same time, if I had known what you intended to do and you had asked me, I think I could have managed to concoct a more satisfactory definition than the one you quoted. For example, I think the following would have been superior and would have secured fewer doubtful replies: 'Operation is to be understood in the sense of any consciously directed and repeatable activity'".

In view of the above statement from Bridgman, it was decided to carry through a new experiment, in June, 1953, substituting his new definition instead of the one ascribed to him in the first experiment. The radical operational definition was also revised. Thus, the Webster and the moderate operational definition remained as before, but the two middle definitions, as used in the new experiment, stood as follows:

2. Bridgman wrote, in 1953:

"Operation is to be understood in the sense of any consciously directed and repeatable activity."

3. A definition which might be acceptable to radical operationists is as follows:

"An operation is any overt behavior of a biological organism, in a material environment, performed by voluntary (striated) muscles."

The instructions used in both experiments were as follows. After the Webster definition, the judges were requested:

"On the basis of that definition, please place, in the Webster column, on the next page, opposite each of the 26 terms, a 'Yes' if you think that term represents an opera-

tion under the Webster definition; write 'No' if you think not; and write '?' if you are not sure."

After the Bridgman definition came the following request:

"Please cover up the Webster column, and then, in the Bridgman column, opposite each of the 26 terms, place a 'Yes' if you think that term represents an operation under the Bridgman definition; write 'No' if you think not; and write '?' if you are not sure."

The instructions after the other two definitions followed similar lines.

As judges in the second experiment, the 30 most reliable judges among those who cooperated in the first experiment were asked to serve, and 24 of them complied. In addition to these, 31 sociology students in Duke Summer School were asked to serve, all but one of whom complied, making a total of 54 judges.

CLASSIFYING THE JUDGES AS TO THEIR
RELIABILITY

Past experience has shown that the performance of various judges who consent to cooperate in a project of this sort varies considerably in its accuracy and consistency. In the first experiment of the present inquiry, a preliminary tabulation of returns showed that, among the 80 questions asked (20 phenomena tested by each of four definitions) 41 received identical answers from 80 per cent or more of the respondents. For example, 82 per cent said that "to blink when a ball grazes one's eye" is an operation *under the Webster definition*. It was assumed that, in each of these 41 cases in which over 80 per cent of the respondents agreed in their answers, the disagreeing answers were in most cases due to (1) carelessness, or (2) to failure to apply with care the instructions and the definitions, or (3) to lack of comprehension, or (4) to adoption of conceptions not generally accepted. Each of the returns was therefore scored, deducting two points whenever a student answered "yes" when 80 per cent or more of the total respondents answered "no" (or vice versa), and subtracting one point whenever a respondent entered a question mark when over 80 per cent of the total answered "yes" or "no."

Scoring on the above basis, 13 of the 51 students had perfect scores, 5 scored -1, 10 scored -2, 22 scored between -3 and -12, and one scored -26. This lowest-scoring student said that 19 of the 20 phenomena were operations under each of the four definitions. This involved such flagrant ignoring of instructions, and produced a score so far out of line with the other 50 returns, that this one return was excluded. The remaining 50 students were divided into 25 "better scorers" and 25 "poorer scorers," putting three randomly selected -6-scorers into the lower group.

In the second experiment, using a similar method, 30 of the judges scored 0, -1, or

TABLE 1. MEAN UNRELIABILITY SCORES ON PHENOMENA NOT USED IN RATING JUDGES

	First Experiment	Second Experiment
Better Judges	67.6	57.4
Poorer Judges	85.4	72.7
Difference	-17.8	-15.3
C.R.	3.8	2.4
P0001	.018

-2, and these were grouped as "better" judges. The other 24 scored -3 or lower, and were classified as poorer judges.

In order to test the validity of these classifications, the reliability scores obtained for the four definitions, as applied to phenomena *not* used in rating the judges, were compared in the returns from the "better" and from the "poorer" judges. The contrasts are shown in Table 1.

On the basis of the above comparisons, it is taken as established that the "better" judges in each of the two experiments actually did do more trustworthy work.

RELATIVE UNRELIABILITIES OF THE VARIOUS DEFINITIONS

The findings of the two experiments, on the basic question of relative reliabilities, may be summarized as in Table 2.

Major conclusions emerging from Table 1 include the following:

1. The moderate operational definition is the most reliable, according to every one of the five comparisons shown in the table. Its superior reliability as compared with the

Webster definition, as measured by Student's *t*, would occur by random sampling less than once in hundreds of thousands of such experiments. The superior reliability of the radical operational definition over the Webster definition is also highly significant in both experiments, but it is less than the superiority of the moderate definition.

2. The Bridgman₁ definition, as tested by the better judges in the first experiment, was found to be significantly less reliable than the moderate operational definition. Such a difference would arise by random sampling less than once in 200 such experiments. But, as Bridgman predicted, his second definition, as tested by the better judges in the second experiment, was much less unreliable than his first one—with an unreliability score of 29.8 as compared with one of 45.0 in the first experiment. This is particularly notable in view of the fact that the second experiment used 14 ordinary undergraduates among its 30 better judges, whereas all the judges in the first experiment were graduate fellows or graduate assistants, selected for superior scholarship.

3. The Bridgman₂ definition specifies that repeatability is to be taken as essential to the concept of operation. To test the effect of this proviso, four phenomena were inserted in the second experiment which are either unrepeatable by the same operator, or of dubious repeatability—namely numbers 2, 4, 21, and 24 in Table 4, relating to murdering one's long-time spouse, dying deliberately on a sinking ship, amputating the left foot of one's own father, and basing the discovery of a lifetime on data collected during 50 years. If these four items are excluded, the unreliability index of the Bridgman₂ definition goes down to 18.5—only 4.7 points higher than the moderate operational definition. With the number of phenomena used, this difference in mean unreliability scores is not statistically significant.

The question arises whether repeatability is actually a suitable differentia for the concept *operation*. In discussions of operationism, it is customary to specify that operations employed in defining concepts shall be repeatable, implying that some operations may be unrepeatable. It would seem more suitable to insert the requirement of repeatability in describing operational procedures, rather than including that trait as part of

the concept of *operation*. Judging from our second experiment, it seems that such a course would eliminate part of the confusion which students experience in attempting to apply the definition.

4. The revised radical operational₂ definition also proved to be more reliable than the radical operational₁ definition. This change in the radical definition produced an improvement also in the score obtained from the poorer judges—which was not the case

ationism emerged in part from logical positivism, which "was an attempt to get back to basic data and thus to increase agreement and diminish the misunderstandings that come about from unsuspected differences in meanings."⁴ Stevens, in 1934, presented operationism as "the revolution that will put an end to the possibility of revolution" in psychology by providing "the rigor of definition which silences useless controversy."⁵

TABLE 2. UNRELIABILITY SCORES OF THE DEFINITIONS TESTED IN TWO DUKE EXPERIMENTS

Definition	First Experiment		Second Experiment	
	Better Judges	Poorer Judges	Better Judges	Poorer Judges
Webster: "Operation: An action."	60.4	73.5	61.7	71.6
Bridgman ₁ : "Operation is to be understood in the sense of any conscious activity."	45.0	46.2
Bridgman ₂ : "Operation is to be understood in the sense of any consciously directed and repeatable activity."	29.8	53.7
Radical operational: "An operation is any action by a biological organism, performed by voluntary (striated) muscles."	28.2	66.8
Radical operational ₂ : "An operation is any overt behavior of a biological organism, in a material environment, performed by voluntary (striated) muscles."	19.1	49.5
Moderate operational: "An operation is any physical or mental change (from that which otherwise would have occurred) which the operator produces intentionally, in himself or in his environment."	8.3	26.3	12.7	47.5
Applied to 22 phenomena—omitting four whose repeatability is open to question:*				
Webster	65.6
Bridgman ₂	18.5
Radical operational ₂	17.6
Moderate operational	13.8

* The omitted four are items numbered 2, 4, 21, and 24 in Table 4, concerned with murdering one's long-time spouse, dying deliberately, amputating one's father's left foot, and basing the discovery of a lifetime on data collected during 50 years.

for the second Bridgman definition. Even after their improvements, however, the moderate operational definition proves to be more reliable than either the Bridgman₂ or the radical₂ definition.

As applied to the specific problem of defining the term *operation*, the claims of radical operationists are at least rendered dubious by the above findings. These radicals have asserted that their behavioristic form of operationism would eliminate misunderstandings from scientific discussion. For example, Boring said, in 1945: "The primary advantage of operational definitions lies in the unification of science and the resolution of controversy."³ In 1950 he said that oper-

But when tested operationally, it becomes clear that the radically empirical type of operationism shows no signs of being the least ambiguous. The Bridgman₂ definition, based upon "consciously directed" activity, is almost as reliable as the radically operational definition (when the question of repeatability is eliminated) and the moderate

³ Edwin G. Boring, "The Use of Operational Definitions in Science," *Psychological Review*, 52 (1952) pp. 243, 269.

⁴ E. G. Boring, *History of Experimental Psychology*, 2nd Ed., 1950, p. 655.

⁵ S. S. Stevens, "The Operational Basis of Psychology," *American Journal of Psychology*, 47 (1934) p. 323.

operational definition, based on intentionally produced changes, whether physical or mental, is the most reliable, on all the tests made in these two experiments.

A more general significance also emerges from these comparisons of reliabilities. Even as applied by the better half of those responding among Duke graduate fellows and assistants in the selected departments, the most reliable definition produces about one-twelfth of the possible number of disagreements in classifying the phenomena used in this experiment. The poorer half of the judges, when using the dictionary definition, disagree with one another nearly three-

AGREEMENTS AND DIFFERENCES OF MEANING

In the light of the above quantitative analysis of relative reliability, it now becomes pertinent to break down the items on which these statistics have been based, and to explore the areas in which understandings and misunderstandings appear to have arisen.

The Bridgman₂, Radical₂, and Moderate definitions are the ones with the highest reliabilities; the agreements and disagreements which they produce, in classifying the 26 phenomena, are shown in Table 3.

It will be seen that all these three definitions agree that simple muscular actions,

TABLE 3. AGREEMENTS AND DISAGREEMENTS AS TO CLASSIFICATION OF SELECTED PHENOMENA, UNDER THE BRIDGMAN₂, RADICAL₂, AND MODERATE OPERATIONAL DEFINITIONS OF OPERATION, AS AGREED BY THE MORE RELIABLE JUDGES IN THE DUKE EXPERIMENT *

Agreements	Disagreements
<i>The following ARE operations:</i>	<i>The following ARE operations under the moderate definition, but under the radical definition they are NOT:</i>
10. To copy a number 13. To write a memo 20. To measure with a ruler	1. To add two numbers mentally 3. To decide what number comes next in the series: 2, 6, 18, . . . 9. To formulate a question mentally 11. To change one's mind 15. To plan 18. To decide which of two shades is darker 23. To invent mentally 26. To try to stop worrying
<i>The following are NOT operations:</i>	
5. To fail to go to sleep 7. To keep thinking of something one is trying to forget 12. To dream of being suffocated 14. To feel pain 16. To shift one's attention involuntarily 19. To breathe unconsciously 25. To feel guilty	

* Disagreements by not more than 20 per cent of a given group are not regarded as altering the majority opinion.

quarters of the possible number of times. If this major degree of disagreement occurs under fairly rigorous experimental conditions, what is presumably occurring when a professor is lecturing to an average class of undergraduates, or when discussion is taking place? It seems likely that a large proportion of all such discussion is misunderstood by the participants.

Indeed, it seems reasonable to raise the question whether any considerable number of those who have published and read articles on operationism in learned periodicals have understood each other more than a minor fraction of the time. The doubt arises persistently whether even the leaders have achieved, and consistently maintained, clear-cut, operational concepts at crucial points.

even when involving mental aspects, are operations, and that simple observations, as well as unconscious activities, are not. The major difference between the Bridgman and Moderate definitions on the one hand and the Radical one on the other, is that the latter excludes purely mental actions from consideration as operations.

The major respects in which these three definitions have been confusing—or have failed to produce agreement—in classifying phenomena can be seen by studying Table 4.

CONCLUSIONS

One of the major announced objectives of operationism has been to promote agreement among scientists. The extent of agreement achieved can be measured, at least in some of its phases, by operational tests of the

reliability of definitions. The present experiment has shown:

1. That greater reliability in definitions is greatly needed—if the definitions used here are at all representative.

2. That higher reliability can be attained by a moderate operational definition of the term *operation*, which includes mental operations, than by a radical operational definition which seeks to exclude everything except overt behavior activated by striated muscles.

3. That if the moderate definition is accepted, radical operationism is thereby excluded.

4. That Bridgman's first definition, while seeking to bring in mental operations, was not formulated with sufficient operational precision to attain an acceptable level of reliability, and that his second definition, while greatly improved in reliability, introduces confusion in the minds of even the better judges by including the requirement of repeatability. Such confusion could be avoided by stating the requirement of repeatability as a specification in operational procedure rather than as a part of the definition of the term *operation*.

TABLE 4. UNRELIABILITY INDEXES OF FOUR DEFINITIONS OF "OPERATION" AS APPLIED TO EACH OF 26 PHENOMENA, BY 30 BETTER (B) AND 23 POORER (P) JUDGES

Phenomena	Webster		Bridgman ₂		Radical ₂		Moderate	
	B	P	B	P	B	P	B	P
1. To add two numbers mentally.....	y*71	y 96	y 0	y 17	n 0	n 57	y 0	y 56
2. To murder deliberately the spouse with whom one has lived for 20 years.....	y 13	y 0	y 99	y 94	y 13	y 17	y 13	y 0
3. To decide what number comes next in the series: 2, 6, 18,	y 84	y 98	y 0	y 0	n 0	n 57	y 0	y 62
4. To die by deliberately remaining on a sinking ship	y 75	y 57	n 81	n 89	n 96	n 98	y 13	y 17
5. To fail to go to sleep.....	n 68	n 87	n 36	y 98	n 25	n 67	n 40	n 70
6. To blink when a ball grazes one's eye.....	y 13	y 56	n 25	n 76	n 89	n 98	n 0	n 51
7. To keep thinking of something one is trying to forget	y 99	n100	n 41	y 94	n 0	n 24	n 41	n 51
8. To see two autos collide accidentally.....	n 98	y100	n 64	n 68	n 60	n 67	n 13	n 56
9. To formulate a question mentally.....	y 89	y100	y 0	y 17	n 0	n 32	y 0	y 57
10. To copy a number.....	y 0	y 17	y 0	y 17	y 0	y 24	y 0	y 45
11. To change one's mind.	y 89	y100	y 13	y 56	n 0	n 57	y 13	y 16
12. To dream of being suffocated.....	?100	n 89	n 0	n 56	n 0	n 8	n 0	n 51
13. To write a memo.....	y 0	y 17	y 0	y 17	y 0	y 17	y 0	y 45
14. To feel pain	n 64	n 80	n 36	n 96	n 0	n 38	n 13	n 67
15. To plan	y 78	y 94	y 0	y 0	n 13	n 83	y 0	y 32
16. To shift one's attention involuntarily.....	y 98	y 98	n 0	n 62	n 0	n 32	n 13	n 17
17. To notice immediately, on first seeing a person, that he or she is very attractive....	n 96	y 90	n 96	y 82	n 13	n 62	n 72	n 98
18. To decide which of two shades is darker....	y 84	y 95	y 0	y 24	n 13	n 67	y 0	y 51
19. To breathe unconsciously	y 36	y 76	n 0	n 0	n 74	n 72	n 0	n 8
20. To measure with a ruler.....	y 0	y 0	y 0	y 17	y 0	y 0	y 13	y 63
21. To amputate the left foot of one's own father	y 0	y 8	? 99	n 97	y 0	y 43	y 0	y 32
22. To cry "Ouch!" at the instant one accidentally hits one's thumb with a hammer....	y 13	y 68	n 46	n 68	y100	?100	n 36	n 45
23. To invent mentally	y 89	n100	y 0	y 24	n 0	n 45	y 0	y 24
24. To achieve the discovery of a lifetime by mentally analyzing data which one has spent 50 years in collecting.....	y 72	y 85	y 88	y 99	n 0	n 68	y 0	y 67
25. To feel guilty	n 81	n 51	n 51	n 91	n 0	n 16	n 50	n 76
26. To try to stop worrying.....	y 93	y 99	y 0	y 38	n 0	n 38	y 0	y 74
Mean	61.7	71.6	29.8	53.7	19.1	49.5	12.7	47.3
Variance	1326	1159	1334	1260	1078	758	356	530

* y means that the majority of the judges classified the phenomenon (under the definition in question) as an operation (answering "yes");

n means that the majority classified the phenomenon as not an operation (answering "no");

? means that the yes and no votes are evenly balanced.

SOME COMMENTS ON THE STATE OF THE GENERAL THEORY OF ACTION

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IN view of the fact that two books¹ in which I have had an important hand which were published about two years ago, have lead to considerable comment both in this *Review* and elsewhere, the Editor of the *Review* has kindly invited me to "have my say in the matter." In considering how this opportunity could be used most effectively it has seemed best to focus attention on two things, namely clarification, and the placing of the work in perspective, rather than attempting to answer criticisms in detail. Reference will be made to a number of these criticisms, but I feel that the problem of communication and understanding should in an important sense take precedence. Furthermore, considerable additional work has been done since the two books in question were written. A brief report on some aspects of this work may help to clarify some of the points of difficulty.

The first important point I would like to make is that, while I, and I think several of my colleagues, felt that the two books in question documented what was in certain respects a considerable advance in the differentiation and integration of the conceptual scheme we have been calling the theory of action, they were in no sense meant to suggest any fundamental break in the continuity of theoretical development in the field as a whole; we feel that they stand in the most intimate relation to a great deal of work done before, and going on concurrently in other circles. This of course includes both sociology and the neighboring fields of psychology and anthropology.

With respect to my personal orientation as a sociologist, I am of course aware of the fact that through the double circumstances of having come into the field through economics, and having received my graduate

training in Europe, certain European sociologists have exercised the most conspicuous and direct influence on my theoretical thinking. A main reason for my early interest in the work of Max Weber, and later that of Pareto, lay in the fact that these men dealt constructively, in my opinion, with the problems concerning the relation between economic and sociological theory which underlay the "institutionalist" controversy of the time in American Economics. In my opinion the then dominant "Veblenian" group among the American institutionalists failed conspicuously to solve these problems while others, like Wesley Mitchell, seemed to me to be sheer empiricists who essentially abandoned the attempt at theoretical analysis in favor of description of statistical trends. Furthermore, it was only after I became fully aware that Durkheim, in his study of the division of labor, was dealing with essentially the same problems, that I really felt I could adequately understand and evaluate Durkheim's work.

This economic and European background has undoubtedly given a certain "slant" to my work, and habits of using terminology, which has perhaps made communication with some of the older traditions of American sociology more difficult than it would have been had I had a regular American graduate training. But I have been increasingly aware of the extent to which many of these American writers were dealing with cognate problems, though often in such different terms that translation was not easy. In the background I found Sumner important. Of the American writers of the last generation, however, I think I have profited most by the work of W. I. Thomas and G. H. Mead, the latter particularly in connection with recent phases of my own work. Both have been most important in helping to build what for me are the critically important bridges between sociology and psychology. In a *theoretical* sense I have found both Cooley and

¹ *Toward a General Theory of Action*, Edited by Talcott Parsons and Edward A. Shils, Harvard University Press, 1951, and *The Social System*, by Talcott Parsons, The Free Press, 1951.

Park highly suggestive, but not providing the same order of *specific* conceptual developments with which to work constructively. Simmel's importance as a bridge between European and American theory is of course very great. It is quite frankly my opinion, however, that the American tradition of that generation did not produce a constructive *theorist* in the strictly technical sense who had the stature of either Max Weber or Durkheim.

If I made one contribution, in my own earlier work, beyond those made by these European writers themselves (including of course Pareto and Simmel) it was the demonstration that all of these men, and certain others who were called economists, e.g., Sombart, had been converging on a *single* theoretical scheme. This was of course not known at the time—for me it was an emergent finding which became clear only late in this period of my work. It was not to be found in the secondary literature, which I canvassed thoroughly. The predominant tendency was to allocate these writers to various different "schools." I am quite sure that a careful critical study of the principal American theorists of the time, which to my knowledge has even now not been made in the requisite terms, would reveal that they could all be placed, with relative ease, in terms of the same general scheme.

It goes without saying that anthropological and psychological influences have also been prominent in my own background but I will not take space to detail them here.

One more major theoretical influence should be briefly mentioned. Though the concept of system has come into the social sciences through several channels, in my own case first through economics, this influence was greatly reinforced by contact with its use in biological science, notably physiology. Building on knowledge of biology gained in an undergraduate major, contact with the late L. J. Henderson, both as an interpreter of Pareto and in other ways, stamped the importance of the concept of system as a theoretical tool indelibly on my mind. This was again reinforced by interest in the work of W. B. Cannon, especially his book, *The Wisdom of the Body*.

I have taken the space to sketch this series of theoretical interests and contacts, in order

to recall to the reader the way in which the theoretical work under immediate discussion fits into the background of the relevant intellectual currents of our time. To me, in surveying this background again, the overwhelming impression is that of the unity and coherence of the main movement. It is true that certain groups of writers on the borderlines of sociology have taken positions which, whatever their *relative* justification in the polemical situations of the time, have proved either to be untenable in themselves, or so remote from the interests of sociological theory as not to be positively useful to the task of the sociological theorist. I should be inclined to put in this category first, certain aspects of the "institutionalist" position in economics, second, the extreme at least of "instinctivism" in psychology, including some interpretations current in the psycho-analytic camp, third, radical behaviorism in psychology, including both the Watsonian repudiation of the "subjective" and the attempts, some still current, to generalize strict "S-R" theory into a theory of *all* social and cultural behavior; finally, fourth, some of the versions of "culturology" of which different variants are exemplified by the work of Ruth Benedict and of Leslie White. This does not mean at all that there are not components in any and all of these movements which are positively useful to sociology, but their *general* structures as theoretical positions are from my point of view no more admissible and usable as schemes in terms of which to unify a theory of action or behavior, which *includes sociology*, than was the utilitarian scheme which was dominant in the later nineteenth century. Very broadly these, to me "deviant," attempts to synthesize, represent on the one side "positivistic" schemes, on the other side "idealistic" ones.² With these qualifications, however, it seems to me that the general movement of theoretical development has clearly been in line with the scheme which I and my colleagues have called the "theory of action" as an emerging and as yet of course, very incomplete, single body of theory comprising the whole range of the

² Cf. my *Structure of Social Action* for an explanation of these terms, and also of the utilitarian scheme.

sciences of action, or to use the Ford Foundation's term, the "behavior sciences."

For some purposes of exposition and some types of analysis it is convenient, even sometimes necessary, to separate out the problems of conceptual structure from those of empirical reference in a body of scientific thought. It has, however, been my contention from the beginning,³ that the theoretical development itself simply could not be understood apart from the continual process of interaction between theoretical reasoning and empirical observation. Sometimes the empirical problems have concerned the interpretation of broad features of the structure of large-scale societies, such as the problems of "capitalism" and of "contractual relations" as they concerned Weber and Durkheim, or of the "circulation of elites" as it concerned Pareto. Sometimes, as in the case of Durkheim's famous study of suicide rates, it concerns statistical data in relation to more specific aspects of these very broad problems. Sometimes it has concerned anthropological studies of the structure of kinship relations or of magical and religious practices in non-literate communities, as in the work of Malinowski, Firth, Kluckhohn. The program of community studies has of course carried this type of empirical work into our own society, sometimes under directly anthropological auspices. Empirical research also may take the form of study by direct observation and interview of selected crucial institutionalized situations in a society, as in the case of my own work on medical practice. There has of course been an immense amount of clinical observation of individual personalities, in therapeutic and other kinds of situations, and a rapidly developing study of attitudes from carefully sampled populations.

These are only a few of the many types of empirical work which concurrently, and in interaction with theoretical development, have been going on continually. It is most important to recognize the diversity of different types of study, their places on a macroscopic-microscopic range of levels, their varying degrees of technical sophistication, etc. I would like particularly to emphasize that studies such as Weber's comparative

work in the field of the sociology of religion and Durkheim's analysis of the data about Australian totemism, both of which simply made the most of available published sources, were *empirical* studies in the strictest and highest sense. The very rapid development of techniques of empirical research in our own generation, has not constituted the *beginning* of empirical work, but has immensely widened the field and accelerated the pace of empirical discovery by putting far more powerful tools in the hands of the researcher than he possessed before.

I hope I have successfully made clear that, in my opinion, the current levels of the general theory of action rest on solid foundations in the development in our fields, foundations both of theoretical thinking and of empirical research in the closest interdependence with each other. Now let us turn to direct consideration of the present state of our theory, both as documented in the two books under discussion and as having developed farther since their publication.

Perhaps the best way to introduce this discussion is explicitly to raise the question, what may legitimately be considered to be *new* in this level of theoretical work? Since I have just been stressing its continuity with previous work this becomes a particularly important question. Professor Ellsworth Faris in his review of *The Social System*⁴ seems to interpret me and my colleagues as resting a claim to originality mainly on "the discovery of the combination of interdependence and independence of personality, culture and system (organization)." In the sense in which he imputes the claim to us, of course he is right in repudiating its newness. But I think he misunderstands the sense in which we feel we have achieved something new. Of course, in the history of theory which I have just reviewed, certain highly significant levels of insight into these connections have been commonplaces throughout the period which is directly relevant, and many of them go much farther back. Certainly Cooley, Thomas, Mead and Park, like Pareto, Durkheim and Weber, had what we may call the "broad" insight, and in different ways contributed important

³ Cf. *Structure of Social Action*, Chapters I and XVIII and *passim*.

⁴ In *American Sociological Review*, Feb. 1953, p. 106.

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specifications and developments at many points.

As in so many other cases in the history of science, the important point is not a matter of the broad insight in this sense, but of the level on which this insight has become incorporated into a differentiated and integrated conceptual scheme, which both does justice to known fact—as an instrument of codification—and makes possible a higher level of generality of formulation and analytical reasoning than before, including the revelation of areas of ignorance and the formulation of relatively specific hypotheses. It is then quite true that in our opinion the focus of the theoretical contribution lies in the interrelations of these three foci of the organization of action, but it is not the fact that we understand that they are in general closely related, but the detailed way in which we analyze those interrelationships that we consider our contribution to lie.

This contribution may be considered to lie on two main levels. The first is that documented in the *General Statement*⁵ which opened our collaborative volume, *Toward a General Theory of Action*. This statement was explicitly the result of an attempt among nine men, with rather widely different backgrounds and views on various subjects, to find the greatest possible measure of *common ground*. It was then completely understood that there would be further areas with reference to which we would part company on the basis of differences of interests, of presuppositions, of emphasis or, in a few cases, of direct and explicit substantive disagreement. In the circumstances it would be expected that the agreement would be on a rather high level of generality.

In retrospect I should consider the following four points to constitute the most important aspects of that agreement: The first is making the existence of a common frame of reference of which the keynote is the *relational* focus on actor and situation, quite explicit. This essentially eliminates, in one major respect, the old "behaviorist" controversy, so far as we are concerned. Put as sharply as possible, whatever differences of emphasis and interest there may be between a Sears, with his background in Hullian S-R psychology, a Tolman with his

animal psychology interests, a Murray with his modified psychoanalytic background and primary interest in personality, a Kluckhohn with his "configurational" interest in culture, and Shils' and my own social-system emphasis, is explicitly held *not* to involve the kinds of differences of basic frame of reference which mean that discussion which gets below the surface must focus mainly on justification of one's frame of reference without ever getting to common substantive empirical and theoretical questions.

The second important point is building certain of the broad fundamentals of "behavior psychology," in a sense broad enough to include both Tolman and Hull, into this frame of reference. Then many of the old controversies as to whether or not such a category as "purpose" has any place in "scientific psychology" or whether for the anthropologist or sociologist utilizing any of psychology at this level may not lead him into fatal basic fallacies, can, for our purposes, be considered to be obsolete and need no longer worry us in their *general* form.

The third, and for present purposes exceedingly crucial area of agreement, is that with respect to the concept of social *interaction* and its critical significance for *all* the disciplines concerned. Though a psychology relatively independent of the concept of interaction can be worked out up to a point, we agreed that this category, with its peculiar emergent phenomena of the complementarity of expectations and "double contingency," is essential to the analysis of *all* the higher forms of organization of action alike, in their social system, personality and cultural aspects. In certain respects of course, this insight has been the stock in trade of the sociologists and the social psychologists in the Cooley-Mead tradition, but it has *not* been common to the "behavior sciences" as a whole. It seems to me that its significance, on the relatively elementary levels included in the General Statement, lies primarily in bringing into relation with this more "sociological" tradition, above all the psychologists in the "experimental" tradition, and to a lesser extent, in the psychoanalytic tradition. But even some anthropologists have tended to "by-pass" the phenomena of interaction.

Finally, fourth, we feel that the statement

⁵ Part I, Chapter I.

related these levels of theoretical consideration to the primary fields of interest of sociologists and anthropologists in social systems and culture, by indicating, in a general way of course, how both cultures and social systems could be shown to develop and change in the processes of goal-oriented behavior in *social interaction*. The fact that cultural patterns are to be conceived both as institutionalized in social systems and as internalized in personality systems is the keynote of this point of view.

It is almost obviously true that *none* of the components which has gone into this extremely general synthesis is new. But to bring all of them together in explicit and orderly relations to each other and in such a way as to be subscribed to with only two dissents by leading proponents of all the main disciplinary traditions involved, is something new. Of course no such document can be considered in any way as "definitive." But it is our hope that this can help substantially to prevent reversion to the old levels of sterile controversy. In short, we conceived this as a contribution to the elimination of the "war of schools" as a dominant feature of the contemporary social science scene.⁶

⁶ Perhaps this is the best point at which to comment on certain questions raised by Professor Swanson (*American Sociological Review*, April, 1953). Swanson contends that the scheme of "orientation" of action presented by Shils and myself becomes "almost exclusively intraorganismic" (p. 131), tending to revert to a "faculty psychology" of an earlier period. This difficulty he feels in turn to underlie our failure to derive the "conventional sociological concepts . . ." which "are the very ones that tend to require some uniting of organisms with the environing conditions set by other organisms." We fail, that is, to take due account of social interaction.

I feel that this interpretation is based on misunderstanding. This starts, I think, with failure to grasp what I listed above as the first fundamental basis of the agreement of our General Statement, namely that we are dealing in a theory of action with a *relational* system. It is *never*, from this point of view, legitimate, except for special purposes of analytical abstraction, to think of an actor apart from a situation and for almost all purposes of action theory, by far the most important objects in the situation are other actors, i.e. "social objects". The system with which we are dealing is therefore the system of *relationships* between actors and objects which, when social objects are involved is a system of social interaction. "The organism" is not *technically* part of the system at all, but is

In *Toward a General Theory of Action* we attempted to make a clear distinction between the area of common agreement included in the General Statement, and an order of theoretical construction which was to be treated as exposition of the more individual views on their own responsibility of the particular authors. Shils' and my monograph "Values, Motives and Systems of Action" belongs in this latter category, as does of course my own *Social System* and a variety of subsequent writings, notably my contributions to *Working Papers in the Theory of Action*.⁷

Let me repeat: all of what we treated in the General Statement as common ground is assumed as underlying this more special and technical development. This includes the goal-orientatedness of action interpreted as constituting a system of *relations* between actor(s) and situation, the fact that this involves processes which have cognitive, "cathectic" and evaluative aspects, the fundamental importance and the essential characteristics of social interaction and, finally, the fundamental importance of culture to both personalities and social systems, including the conception of its internalization and institutionalization.

If these fundamentals are treated as assumed, then it is correct, as several reviewers have noted, that the "core" of the more personal theoretical contribution which Shils and I have made is to be found, in our opinion, in what we have called the "pattern variables." These, it will be remembered, are five dichotomous "dilemmas of choice" which, we have contended, define *alternative directions* in which the orientation of action can go. These are, in a different order from that in which they were originally presented, (1) Specificity—Diffuseness; (2) Affectivity—Affective Neutrality; (3) Universalism—Particularism; (4) Quality—Performance (earlier formulated as Ascription—Achievement), and (5) Self-Orientation—Collectiv-

treated first as motivational energy source, second as providing a set of objects which are classifiable as facilities and reward-objects to the actor, both ego—relative to his own organism—and alter. From this point of view the personality is not a set of "properties" of the organism, but is a *system of action*. We put orientations into the personality, not the organism.

⁷ By Talcott Parsons, Robert F. Bales and Edward A. Shils, Free Press, 1953.

ity-orientation. We contended that, on the requisite level of abstraction this constituted an exhaustive list. We further contended that the variables did not constitute only a "list" but were grouped to form a scheme which had the property of "symmetrical asymmetry," namely 1 and 2 form a pair, 3 and 4 form a pair, and the fifth is unpaired.

Dr. M. Brewster Smith in his review of *Toward a General Theory*⁸ has formulated four questions about the pattern variables which are most pertinent and can serve as a basis for organizing the present discussion. These are: (1) Are the pattern variables in fact an exhaustive list? (2) Are they appropriately regarded as dichotomous? (3) Do the systems of classification which they generate "fit" the needs of personality, social and cultural theory? and (4) Do the pattern variables contribute to a satisfactory general theory of social behavior?

In order to approach the first two of Dr. Smith's questions it is necessary to distinguish clearly between these two, the "general frame of reference" level and the pattern variable level of conceptualization. How can the distinction between and relations of these two "levels" be stated? I think the best way of putting it is to say that the "general frame of reference" concerns the basic characteristics of "action" as a category of phenomena, without special reference to the nature and problems of the systems in which the relations of units of action to each other are organized. The pattern variable level on the other hand *makes explicit the problems of relationship of action units in systems.*

On the more general frame of reference level, we have said three main things which are relevant to the concept of system. The first is that any system of action is *relational* relative to the "components" conceived to make it up, namely "actors" and "objects." The second is that these may be conceived to "interact" which is, I think, in the most general form to say that *the same concrete unit may be conceived as both actor and object in different perspectives.* The third thing said is that the relevant systems are "boundary-maintaining," in this respect

being more like organisms than like the systems of classical mechanics. They maintain a difference between states "internal" to a given system and those in the situation or environment of the system. They have the property of "integration," interpreted as involving the control both of internal states and of boundary-processes in such a way as to maintain this internal-external differential.

As I see it now, the pattern variables are a way of making explicit and formulating in a technical and orderly way, the basic frame of reference in terms of which these properties of action at the level of complexity where their organization in system is involved, "make sense." They formulate the "directions of movement," i.e., the basic *modes* of "change of state" of a unit in its relations to others in a system and to the state of the system in relation to the situation external to it. These directions must be taken into account, precisely by discriminating them from each other and ordering them, if the process of action is to be analyzed as process in systems in the classical scientific sense. Alternatively stated, the pattern variables constitute categories for the orderly description and comparative analysis of the "structure" of systems of action as systems.

It is in this context that I wish to discuss Dr. Smith's first question, "are the pattern-variables in fact an exhaustive list?" and also his second "are they properly regarded as dichotomous?"

There is a sense in which we may treat all of the pattern variables as ways of formulating the consequences of the fact that human social action is not a state of Nirvana in the Buddhist sense. It is subject to certain fundamental restrictions, which is almost another way of saying that it is process occurring in systems. These restrictions are of two most fundamental orders. The first derives from the finitude of the temporal existence of man, and therefore the necessity of some process of selection or "decision" about time-allocation, about when to seek direct gratification, when to impose discipline in the interest of future states. The second set of restrictions concerns the implications of the coexistence of many different organized entities of action which may be treated as units or systems

⁸ *Journal of Abnormal and Social Psychology*, April 1953.

according to the perspective. Any given unit of a system, an "actor," is subject to the exigencies of coexisting in at least one system with other units and, in turn, this system is subject to the exigencies of coexisting with other systems which constitute a "situation" to it.

The dichotomous character of the pattern variables derives from the fact that both of these sets of restrictions constitute basic *dilemmas*. With respect to either pair, namely the alternative to "take it now" vs. to give priority to continuity and stability over time, and the alternative to protect the solidarity of the interactive system vs. optimizing relations with the situation external to the system, it is not possible for a process to "go in both directions at once." We conceive these relations logically to be comparable to those of pairs of rectilinear coordinates of a space. The essential point is a very familiar one. Given the x and y coordinates in the usual sense, it is possible to increase or decrease the values of both variables at once only by changing the distance of a point from the point of origin. But this distance is not a function of the relation between x and y but of other variables. So far as the relation between x and y is concerned, the value of x can only be increased by decreasing the value of y and vice versa. This is the sense in which the pattern variables state "dilemmas," that is, are dichotomous. It can be seen that this sense of dichotomous mutual exclusiveness is in no way incompatible with the idea of continuity of variation with respect to such a variable.

If this is correct, then the fact that there is one basic dilemma having to do with time-allocation, and one with "space," i.e., the coexistence of units and systems, makes sense if we mean by it that these two "problems of organization" of units in systems are not reducible to each other. Given a particular unit of organization of action, an actor, no level of "optimization" of the distribution of gratifications over time, however high, will by itself solve the problems of his relations to other units. Similarly, no solution of the problem of integration with other units in a system, or adaptation to objects outside the system will by itself solve his time-allocation problem. This is the sense in

which we conceive the two basic dilemmas to be independent of each other.⁹

What, then, can we say of the significance of the three different groupings of the pattern-variables?, namely to constitute the "object-categorization pair" (universalism-particularism and quality-performance), the "attitudinal pair" (specificity-diffuseness and affectivity-neutrality) and finally the as yet unpaired self-collectivity orientation?

Put very schematically, we are saying that we can and must view any system of action from three different perspectives. The most familiar of these to American common sense is that action "consists" of human personalities, interacting with each other. The second, somewhat less familiar, one is that it is or involves one or more social systems, i.e., systems constituted by interactive relationships (not personalities) and the third, that in both respects any action system is a process which is "normatively" regulated, its common culture is in one respect a system of norms.

Seen in these terms the question of the exhaustiveness of the pattern variable list reduces to two others. The first is whether the basic "functional problems" of systems of action can be reduced to four, while the second is whether there is any fundamental significance in the classification of the three "aspects" of the organization of action which we have called social system, personality and culture. If this is the logic of the problem, then it should follow that the list should contain a sixth dilemma to pair with that of self-collectivity. I have recently come to the conclusion that this should be the case.¹⁰

The four basic functional problems of systems of action we have formulated¹¹ are (1)

⁹ This involvement of a "time-allocation" problem in the reference system (or space) of the theory of action, should not be taken to mean that time ceases to have its usual significance as a further independent variable. The significance of process in time as relating antecedent to consequent state is on a different level from that of the time-reference of alternatives of selection.

¹⁰ This was first seriously suggested by Bales in personal discussion. He should receive full credit for the suggestion, which has on consideration seemed increasingly correct and important.

¹¹ *Working Papers in the Theory of Action*, Chaps. III and V. When the pattern variables were first formulated this basic relation to the func-

"adaptation," i.e., to objects in the situation outside the system, (2) "goal-attainment," i.e., establishment of "consummatory" relations to situational objects—by "instrumental" processes, (3) "integrative," the maintenance of a state of internal "harmony" or absence of "conflict" among the units of the system and (4) "latent pattern-maintenance and tension-management," the maintenance *both* of the structure of the internalized-institutionalized normative or cultural patterns, *and* motivation to conformity with their requirements.

It seems clear that this is an irreducible list, judged by the needs of the frame of reference of action. It has worked sufficiently well, so that I think it is legitimate to place the burden of proof on him who would reduce it farther or expand it.

We now conceive the two pairs of "attitudinal" and "object-categorization" pattern variables, as *each* formulating these four system-problems or *dimensions* as we have called them (*Working Papers*, Ch. III). Each does so from one of the two "sides" or aspects of the relational system which we conceive action to be. This arrangement is shown in Figure 1.

FIGURE 1. SYSTEM PROBLEMS AND PATTERN VARIABLES

System Problem	Attitudinal Aspect	Object-Categorization Aspect
Adaptation	Specificity	Universalism
Goal-Attainment	Affectivity	Performance (Achievement)
System-Integration	Diffuseness	Particularism
Pattern-Maintenance and Tension-Management	Affective Neutrality	Quality (Ascription)

If this interpretation is correct, then, within the same specific system reference, we have not formulated eight independent variables, but only four, which is a great gain from the point of view of manageability. But the two different formulations are none

tional problems of systems was not clear. This only emerged after their relationship to Bales' categories of interaction had been worked out (*op. cit.*, Ch. III).

the less significant, because they call attention to the fundamental fact that, except in limiting cases, we are *always* dealing with the interpenetration of *at least two systems*. Every social system, that is to say, involves in some degrees and senses, the "participation" of a plurality of personalities. The "attitudinal" version of the variables formulates the primary involvement of the personalities regarded as systems, which always transcends any particular role. The "object-categorization" version, on the other hand, formulates the fact that members of a collectivity are objects to each other in the sense that *their relative locations in a system* always transcend the involvement of any particular personality in that system. He is "oriented to" all of them as objects, but he, *in his role*, is only one of these objects and is always differentiated from others in the system. Furthermore, what we have called the "symmetrical asymmetry" of the pattern variable system shows that the organization of these dual system-references relative to each other *cannot* be random, but must involve systematically determinate relations.¹² Further basic significances of the difference of "aspects" are to be found in the distinction, in processes of interaction, between performances and sanctions, and closely related to this, between facilities and rewards.

What now of the fifth pattern-variable, self-orientation vs. collectivity-orientation and its relation to the normative or cultural aspect of systems of action? It has become increasingly clear for some time that this concerned the relations of a given system of reference to other systems. We are aware that a "society" and a total "personality" are limiting cases of the concept system of action. They must in turn be treated as composed of complex networks of subsystems, which are organized in a "hierarchical" order of greater and lesser inclusiveness. Collectivity-orientation then formulates the respects in which membership in a superordinate system is a directly governing consideration for action in or as

¹² There is no space to go into these problems it yet worked out will be found in Parsons, Bales, et al. *Family, Socialization and Interaction Process*, Free Press, late 1953 or early 1954, esp. Chaps. III and IV.

a member of any given subsystem. Self-orientation on the other hand formulates the area within which the norms or interests of the superordinate system are not directly governing, that is where they may be treated only as "regulative" rather than "constitutive" of the relationship in question. In familiar social system terms, in ordinary economic transactions in his capacity as "consumer" in our society an individual is free, within the regulative limits of "fair dealing," to use his income in the manner he conceives to be most advantageous to him. The same individual, however, in his capacity as an administrative officer in an organization (e.g., chairman of a university department) in making up a department budget is positively obligated to give first consideration to the interests of the department as a collectivity, that is as a social system, not to his individual wishes.

All roles in social systems—to use this type for illustration—are governed by the norms of its institutionalized culture. What this pattern variable does is, in the relations of systems to each other as distinguished from their internal affairs, to distinguish between two modes of normative control to which action is subject, those positively defining obligations of membership in superordinate collectivities, and those merely setting the limits of permissible action relative to the superordinate collectivity. If the reference is *only* internal, the cognate category is the integration of the system itself. The inference is that the *cultural* level always involves a reference *beyond* the system which is the focus of analysis.¹³

Within the available limitations of space this is the best answer I can give at present to the first two of Dr. Smith's questions. The terms used differ substantially from those of the original exposition, but I hope they will be clearer. A great deal of analytical work

has gone on in the meantime which has clarified a number of questions which were obscure in the earlier stages of development of the scheme.

I may now turn to the third and fourth questions, whether the pattern variables "fit the needs" of theory in the relevant fields and whether they contribute to a "satisfactory general theory of social behavior." Following the leads of Dr. Smith's exposition under these headings I shall interpret question three to concern the "fit" with current theoretical categories in the respective fields, and question four to concern the empirical usefulness of the scheme. The two questions of course cannot be completely separated, but these two emphases will form convenient points of reference for organizing the rest of the material.

Let us start with the theory of the social system, that is sociology. The first beginnings of the pattern variable scheme came from the attempt to discriminate types of social structure, taking the lead first from Toennies' distinction of *Gemeinschaft* and *Gesellschaft*, and then from some of Max Weber's refinements of this. To me its first great utility was that it made it possible to formulate the precise sense in which the professional role was differentiated both from that of the businessman, and from that for example of the father of a family, thus in Toennies' terms belonged to *neither* of his two main categories, but had properties Toennies ascribed to both.

For many years I have been greatly concerned with the development of categories for the systematic classification of social structures, because of my conviction that without them systematic comparative analysis would not be possible and the levels of generalization accessible to sociology would remain exceedingly low. We would lack, that is to say, the kind of framework which the classification of species and comparative anatomy have given to the biological sciences.

Since the breakdown of the older evolutionary theories we simply have not had such a scheme, indeed some of us have tried to make a virtue out of necessity and act as though the "richness" of the variety of social structures in principle defied analysis, and this was "a good thing." By far the most important attempt was that of Max Weber in

¹³ Perhaps this "reference beyond" can serve as a clue to the formulation of the sixth pattern variable which I now feel is needed to complete the system. I have already suggested that it involves a reference in time which goes beyond the specific current system-problems of the system itself. The essential problem seems to be that of *continuity* in time as a focus of valuation; hence the dilemma may be formulated as simply that between short-run and long-run interests or values. There is no space to go into the problem further here.

the *Theory of Social and Economic Organization*. But this, for reasons which cannot be gone into here, left a great many problems unsolved.

The new level of development of the scheme of pattern variables which came with *Toward a General Theory of Action* and *The Social System* have, in my opinion, made it possible to make a substantial advance toward such a classification of social structures, one which for the first time goes beyond the level reached by Max Weber. This analysis culminates in Chapter IV of *The Social System*. There is no space to discuss it in detail here. It did, however, seem to me that it had proved possible to derive, step by step from the general frame of reference, and from the conception of organization of action in social systems, all of the main types of social structure with which sociologists have regularly been working. I am sure that this could be much more adequately and elegantly done now than it was at that time, and when the time for revision of this book (*The Social System*) arrives, I expect to undertake the job. Hence all I wish to assert now is that this attempt constitutes a substantial advance over what has previously been available in the literature. This, and recognition of the importance of the task for sociology, are the two points I wish to emphasize.

There is, however, one further general point. In my own earlier work I considered the pattern variables to be categories of social structure as such. It was one of the most important insights of the new work under review that they were in fact categories of systems of action in general. Hence their use in order to derive categories of social structure necessarily involves a logical step in derivation which makes the transition from the most general level to that of social interaction. Thus for example affective neutrality could, if referred only to an individual actor, mean simply inhibition, in the immediately current situation, of an impulse to direct gratification. But as a category of the institutional organization of a social system it must mean more than this, it must refer to a generalized obligation, for those in a certain type of role in the social system, to refrain from gratifying impulses which would interfere with a given type of

performance or relationship in the social system. Put in personality terms, mechanisms other than simple inhibition, namely those involving "superego" controls, seem to be essential for this function.

One general remark may be made about the problem of "fitness" in the sense of derivation of concepts in common use from more general sources. This concerns the apparent assumption that it is a valid criterion of the significance of the pattern variables that more concrete concepts should be derivable from them *alone* without reference to the other aspects of the frame of reference and general theory.¹⁴ This is like requiring—and I think the analogy is relevant—that the orbit of a planet should be derivable from the three dimensions of Euclidean space without reference to the categories of mass or velocity. We have *never* contended that the pattern variables as such exhausted the "primitive" concepts of the theory of action. Only on such an assumption is the capacity to derive the kinds of concepts we have been discussing from the pattern variables *alone* a test of the validity or usefulness of these concepts.¹⁵

A few words may be said about the "fitness" of our categories to personality and cultural theory. It was a primary insight of the work now being reviewed that the pattern variables were *not* confined in their relevance to the level of social systems, but were categories of the *general* theory of action. Has this been borne out in the fields of personality and cultural theory? Since I am a sociologist and neither a psychologist nor an anthropologist, I have not devoted as much attention to these fields as I have to the analysis of social systems as such. However, particularly on the basis of as yet unpublished work,

¹⁴ Professor Swanson seems to me to do this in his complaint that Shils and I "do not derive a whole series of concepts referring to organizational forms from these pattern variable dilemmas". (*Op. cit.*, pp. 132-33.)

¹⁵ In addition, it may be noted, Professor Swanson does not make a clear distinction between "do not derive" and "cannot". The former phraseology refers mainly to the specific monograph of Shils and myself. Our space was limited in that document, and we had to take a great many things for granted. Many of them are spelled out in earlier writings, in *The Social System*, and various others in subsequent writings, some of which of course were not available to him when he wrote.

both of others and my own, I do not hesitate to say that it has now been amply borne out.

The first attempts in Shils' and my chapter on Personality¹⁶ and in Tolman's monograph¹⁷ to give psychological meanings to the pattern variables were necessarily crude. A great advance became possible when we understood the relations of the pattern variables to the system-problems as these have been outlined above. On this basis it has, for example, proved possible to show¹⁸ that the main organization of the personality at the completion of the oedipal transition can be characterized as a system of four basic need-disposition units which correspond to the four basic attitude types as derived from the combinations of the attitudinal pattern variables. It is further possible to show that more refined discriminations in orientation to the social object-world, e.g., in peer group and school, are analyzable in terms of the object-categorization types.

It has, furthermore, proved possible to show that this classification of the basic orientations of need-disposition types corresponds directly with Freud's famous structural analysis of the personality into Id, Ego and Superego, if one assumption be allowed, namely that the Ego has two importantly differentiated aspects which we call Adaptive and Integrative, respectively. This distinction is, I think, itself actually made by Freud, e.g., in discriminating "reality-testing" from the "ego-ideal."

There is no space to carry this analysis further here. I must rest content with stating that if these contentions can be validated, and they will soon be available for general examination, they will fully bear out the contention that the pattern variables are indeed relevant to personality theory.¹⁹

¹⁶ *Toward a General Theory*, Part II, Chap. II.

¹⁷ *Ibid.*, Part III.

¹⁸ Cf. Parsons, Bales, et al., *Family, Socialization and Interaction Process*, Chapter III.

¹⁹ Some very important recent work by James Olds (cf. *Psychological Papers in the Theory of Action*, Free Press, late 1953 or early 1954) has shown that even the most elementary levels of "behavior psychology" can advantageously be treated in terms of the conception that such behavior consists in "systems of action". This is true even of the classical "stimulus-response sequence" in both Hull's and Tolman's versions. This, of course is not as such "personality theory" but touches many of its psychological foundations. Olds shows further how these elementary units of be-

The case of "culture theory" is from the present point of view much more difficult than that of either sociological or psychological theory. This, apparently is a function of the fact that the culturally oriented anthropologists have devoted substantially less attention to categorization and classification than have either sociologists or psychologists. There can in the first place be no doubt that our theoretical scheme is in accord with the broad "cultural point of view," if one point of interpretation is granted. This is that such a point of view does not basically challenge the independent significance of the social system as a mode or level of the organization of action. There is a tendency in some anthropological circles to do this, but it is far from universal. If, on the other hand, the primary emphasis is on the "social heritage" which is learned by the individual, there is as such no difficulty. Furthermore, the pattern variables can be shown to formulate modes of organization of symbolic meanings in patterns²⁰ and in this connection I am sure that they can make important independent contributions to the theory of culture.²¹ The latter, however, has not put forward classifications which are directly comparable with that of the pattern variables.²²

In this all-too-brief review of Dr. Smith's

havior come to be organized in larger systems which approach much more closely to what is usually treated as the "personality" level.

²⁰ *Working Papers*, Chapter II.

²¹ This will, I think, be greatly facilitated by developing the implications of the sixth pattern variable which I have barely mentioned above.

²² Perhaps the closest which is current is that of Florence Kluckhohn's scheme of classification of cultural value-orientations. Cf. Florence Kluckhohn, "Dominant and Substitute Profiles of Cultural Orientations: Their Significance for the Analysis of Social Stratification", *Social Forces* (May, 1950), pp. 376-393; also in Hugh Cabot and Joseph A. Kahl, eds., *Human Relations: Concepts in Concrete Social Science*, Harvard University Press, 1953. This classification has several important points of contact with the pattern variable scheme, but as a whole is built on quite different premises. Its main point of reference is not, as in the pattern variable case, to systems of action as such, but it is organized about orientation to general "human problems" as the author puts it. When such differences of points of reference are carefully taken into account it seems to me that there is not likely to prove to be any serious discrepancy between schemes of classification arrived at from a cultural point of view, and the pattern variable scheme.

third question I have necessarily overlapped somewhat with his fourth, "Do the pattern variables contribute to a satisfactory general theory of social behavior?" As noted above I interpret this question mainly in terms of empirical adequacy. I should like to divide my answer into two parts. The first concerns the codification of already available empirical generalizations, the second its use as an instrument for the finding and validation of new empirical generalization.

With regard to the first aspect, I regard recent developments in this conceptual scheme as extensions of the work of codification which I undertook in the *Structure of Social Action*. There the principal empirical generalizations present in the work of Marshall, Pareto, Durkheim and Max Weber were carefully reviewed and where they were not, as for example in certain respects was true of Marshall's view of social evolution, shown to be untenable, they were shown to be subsumable under a common conceptual scheme. This was true for example of Weber's and Durkheim's very different generalizations about the "institutional" factors in an "individualistic" system of market relations, and of some of their generalizations about the relations between religion and social structure, derived directly in the one case from the study of Australian totemism, in the other from the comparative study of the more "advanced" religious systems.

More generally, I see no serious difficulties in codifying the available body of empirical generalization in the field of comparative analysis of the larger-scale social systems in these terms; various attempts in this direction have already been made in my own work.

Another very important work of codification has been made possible by demonstrating the precise relationships between the pattern variables and Bales' categories of the interaction process.²³ This makes it possible to bring into the same general scheme analysis of social systems both on the large-scale and on the small group level. We are so optimistic about the possibilities of this that we are working directly now on a study of

stratification in small groups which is conceptualized in exactly the same terms as have been used in analyzing the stratification of the large-scale society; namely pattern-variable terms.

Closely related to this is the field of the relations between social structure and personality. In a forthcoming publication²⁴ it has proved possible to analyze the process of socialization as a process of social interaction precisely in these terms and through that analysis to carry the use of the pattern variables in the formulation of the structure of personality as a system very much farther than was done before. This is also true of the classification of the mechanisms of personality process which have there been arranged in terms of their relations to the pattern variables.²⁵ In the process it has been possible to relate this analysis quite explicitly and in detail to several current bodies of empirical generalization, notably Freud's scheme of the stages of psycho-sexual development, and Piaget's conceptions of the development from "moral realism" to "cooperation" and also of the development of logical thinking in the child.

Closely related to this is the contact of this scheme with "reference group" theory. Thus the distinction made by Merton and Kitt²⁶ between what they call the individual's "attitudes" and his "self-image and self-appraisal" can be shown to involve exactly the same distinction which has been made between the "attitudinal" aspect of the pattern variable system and the "object-categorization" aspect.²⁷ I think it is not too much to say that the reference group scheme and that of the pattern variables are coming rapidly to be capable of direct mutual translation.

²⁴ *Family, Socialization and Interaction Process*, *op. cit.*

²⁵ *Op. cit.*, Chap. V.

²⁶ "Contributions to the Theory of Reference Group Behavior" in *Continuities of Social Research*, Merton and Lazarsfeld, (Eds.) Free Press, 1950, pp. 73-74.

²⁷ In relation to Merton's work another notable convergence has been that between the classification Merton developed in his "social structure and anomie" paradigm, and the classification of types of deviance developed in *The Social System*. This convergence was noted at that time. Since then the relation to the pattern variables of the deviance classification has been much more fully worked out. Cf. *Family, Socialization, etc.*, Chap. IV.

I am, however, also quite sure that such codification will involve considerations other than those explicitly formulated in the pattern variables.

²³ *Working Papers*, Chap. III.

These examples will have to suffice to indicate that on the level of codification the general scheme of the theory of action, including its pattern variable level, is proving to be a powerful instrument of the unification and generalization of the empirical knowledge we already possess. It is not too much to say that this process of codification has only begun and the prospect is that as more work is done the generality of the scheme in empirical fields will be more and more extensively demonstrated.

Finally, it is possible to say only a word about the scheme as a direct instrument of new empirical research. Here I shall cite only three examples from my own experience. In the first place the first beginnings of the pattern variable scheme were worked out in connection with a study of the institutional pattern of modern medical practice and its relation to the mechanisms of social control.²⁸ For this study, which I think it fair to say achieved results well beyond common sense, the theoretical scheme was an indispensable guide from the stage of formulating the problems on.

Second, in a study of social mobility currently going on in collaboration with Samuel A. Stouffer and Florence Kluckhohn, a long-range systematic attempt is being made to use and develop this type of theory in predicting from the characteristics of families, peer groups and schools, and the place of a boy in them, what place in the occupational system he will come to occupy. In this connection the adult occupational system is being treated as a system of reference groups toward which a boy may be thought of as coming to be selectively oriented. The socialization process in all three of the "socializing agencies" mentioned above is being analyzed as a process of social interaction. The basic point of reference for the family aspect of the analysis, for instance, is the attempt to define the conditions in the family as a system under which a boy will tend to "identify" with his father in the sense of tending to realize the father's values (not necessarily actual status) in his own occupational future. From these reference points then we are attempting to formulate

hypotheses as to the effects of certain selected variations from these conditions.

The third case is the study of stratification in small groups mentioned above. Here we are attempting to study the differences in the stratification of the group and its perception by the members which will be made by conditions which put a premium on instrumental performance functions in the group on the one hand, of integrative functions on the other.

We have taken as our major point of reference the theoretical formulation, made for the large-scale society, that the rank order of valuation of performance types (functions in the system) and that of the distribution among units of facilities and rewards, will tend to coincide.²⁹ This coincidence we hypothesize, is common to all stable social systems, large or small. What will be the *content* of the valued performances, facilities and rewards will, however, vary as a function both of the value-system of the group and of situational and personality factors. Our attempt is to take the value system as the independent variable and study the changes in these categories which result from changes in it. The central hypothesis then is that in so far as instrumental values are successfully inculcated in a group the instrumental leadership functions will have highest prestige and the greatest facilities will be placed at the disposal of instrumental leaders, and similarly with integrative leadership when the values are integrative. Of course we also expect various other features of the stratification system to vary, such as the degree of fluidity or rigidity, and the type of symbolization of prestige. There is no space to develop this farther here. These three examples have been introduced only to make clear that the use of this type of theory in going empirical research is much more than merely programmatic. A good many other examples could also be cited.

In conclusion I would like to leave the reader, I think, with three major impressions about the general theory of action as

²⁸ Reported most fully in *Social System*, Chap. X, but also in several other papers.

²⁹ Cf. Parsons, "A Revised Analytical Approach to the Analysis of Social Stratification" in Bendix and Lipset (Eds.), *Class, Power and Status*, Free Press, 1953.

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a conceptual scheme. The first is that it is not something which has been invented "out of the blue" and thrust into our social science scene, but is very much a development out of the lines of theoretical thinking which have deep roots in both European and American tradition. Appreciation of just how the lines of influence operate cannot, however, be arrived at by superficial comparison of concepts and their definitions, but requires careful critical analysis. When this is carried out I am entirely confident that the continuity with our past will be amply evident.

The second major point is that it is not a static scheme which has been put before the profession to "take or leave" but is a rapidly and dynamically developing body of ideas on a variety of different levels. This of course means that it is unevenly developed in different parts. At the time the two books with which this discussion has been primarily concerned were written, their authors took

special pains to emphasize that they expected further developments to take place. The event has amply borne out this expectation. This rapid development is of course a source of difficulty and sometimes confusion because it makes it so difficult to maintain consistency of terminology. But that it is definite *development*, not aimless wandering, I am fully satisfied.

Finally, the claim that this conceptual scheme is part of empirical science and not just "speculative" is meant with the utmost seriousness. Any reader who takes the trouble to analyze carefully the scope of codification which already has been carried out in its terms should, I think, find little difficulty in convincing himself of this. But further, I have emphasized that it is being used most seriously in new empirical research on a number of fronts. In this respect as in that of theoretical formulation as such, it is to be thought of as a developing conceptual scheme.

JUVENILE REPEATERS FROM TWO VIEWPOINTS

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A RESEARCH pattern often used in studies of juvenile delinquency involves comparing a group of delinquents with either a general population or a matched sample of non-delinquents. The presumption is that those items on which the delinquents differ are either causes of delinquency or else are associated with the causes. The most extensive series of data to be examined in this way appears in the work of the Gluecks,¹ in this country, and, in England, the report of Carr-Saunders and his associates.² A very interesting variation was the use of non-delinquent siblings as the comparison group by Healy and Bronner.³

¹ Sheldon and Eleanor T. Glueck, *Unraveling Juvenile Delinquency*, New York: Commonwealth Fund, 1950.

² A. M. Carr-Saunders, Hermann Mannheim, and E. C. Rhodes, *Young Offenders*, New York: Macmillan, 1944.

³ William Healy and Augusta F. Bronner, *New Light on Delinquency and Its Treatment*, New Haven: Yale University Press, 1936.

In all of these studies, with the exception of the Healy and Bronner, there are the usual findings of residence in high-delinquency areas and poor educational adjustment. In all, without exception, there is evidence of poor family relationships.

Where the delinquent group is so classified on the basis of court action there is always suspicion that selective factors have been at work. As has been stressed by Porterfield⁴ several steps intervene between the detection of a juvenile offender and his appearance in court. At several points along this path, a "good family" may intervene successfully. Thus, the final court group is the residue of a selective process. For example, of the 4,533 boys and girls against whom complaints were registered with the Youth Bureau of the Detroit Police Department in 1952, delinquency petitions for only

⁴ A. L. Porterfield, "Delinquency and Its Outcome in Court and College," *American Journal of Sociology*, 49 (November, 1943), pp. 199-208.

1,028 were filed with the Wayne County Juvenile Court. Thus, the differences between a "delinquent" group and a non-delinquent control group could possibly reflect to some degree the bases of selection rather than "causes" of juvenile misconduct. This possibility has been mentioned in numerous works on delinquency.

There is a possibility that another and more subtle process may affect comparisons between groups involving delinquents. It may be that the very situation of an arrest may alter the family relationships, and set new forces in motion. Writers on delinquency are familiar with the fact that the differences between non-delinquents and delinquents appear greatest where all the delinquents are youngsters in institutions, rather than mere "court cases." The differences are even less if police arrests are the criterion. It has generally been assumed that this progression reflects the selective process mentioned in the previous paragraph. However, this may be only partially true. It should be recognized that for many youngsters there are several police contacts before recourse is had to the courts, and that courts generally try probation several times before sending a child to an institution. All of this means that time is passing, and during that time there can be significant changes in the youngster and his relationships.

The present study was undertaken to obtain a rough indication as to whether such changes did have sufficient existence in fact to make them worthy of more intensive study. Of necessity, even for crude verification, a complex research design is necessary. A comparison was undertaken of two different series of data. Both series contrasted a group of youngsters repeatedly in trouble with the Detroit police with a second group having had only one such contact. In the first series the two groups were compared on the basis of information recorded at the time of their *first* contact with the police. In the second series, the data on the repeaters was taken one year later from records based on their *last* contact, when their progress as repeaters had been thoroughly confirmed. In all cases, the data used were drawn from "history sheets" maintained by the Youth Bureau of the Detroit Police Department for all boys against whom complaints were

made. On the basis of interviews with the boys, visits to their homes, and contact with their schools, specially chosen police officers recorded facts and opinions on over fifty items of information.⁵

The comparison between repeaters and non-repeaters based on first contact we shall term the "predictive" study.⁶ The records of 2,137 boys first observed in 1946 were divided into two groups on the basis of whether or not there was any record of a later police contact at any time in 1947. There were 672 repeaters and 1,465 non-repeaters. Then, the two groups were compared on all items of information recorded at the time of the 1946 police contact. The tabulations were tested for statistical reliability of differences by the chi-square technique. Nineteen items of information proved to be predictive at the one per cent level of confidence; the category positively associated with repeating appears in parenthesis:

1. Condition of dwelling unit (substandard; needs repairs).
2. Overcrowding of dwelling unit (space rated "inadequate").
3. Racial homogeneity of neighborhood (mixed neighborhood).
4. Quality of neighborhood (slum, below average).
5. Frequency of church attendance (irregular).
6. Attitude toward school (indifferent, dislikes).
7. Things disliked about school (subjects).
8. Type of employment (other than newspaper).
9. Way in which parents gave money (on request).
10. Type of entertainment preferred by boy (shows).
11. Adequacy of family income (rated "inadequate").
12. Family ownership of a car (no car).
13. Number of sisters (three or more).

⁵ The author wishes to express appreciation to Senior Inspector Sanford Shoults, Inspector Ralph Baker, and Lieutenant Francis Davey for their aid in making these records available for research purposes.

⁶ Full details of this study appear in William W. Wattenberg, "A Comparison of Repeaters and Non-Repeaters Among Boys in Trouble with the Police in Detroit in 1946 and 1947," *Papers of the Michigan Academy of Science, Arts, and Letters*, Vol. 35, pp. 395-405, 1949.

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14. Intelligence, as estimated by officers (below average).
15. Attitude of boy toward police officers (evasive).
16. Attitude toward parents expressed by boy (dislike).
17. Neatness of appearance (slovenly).
18. "Type" of boy ("Peck's Bad Boy").
19. Congeniality of family (frequent quarreling).

In the second study this procedure was substantially reversed. The records of all boys handled by the Detroit police in 1948 were obtained. On the basis of whether or not the files had evidence of a previous police contact, they were divided into two groups: repeaters and new offenders. The repeaters consisted of all boys for whom there was a record of two or more offenses in 1948 or else a record that in addition to a police contact in 1948 they had been the subjects of at least one complaint in a previous year. The "new offenders" consisted of all boys for whom there was a record of only one police contact in 1948 and none in a previous year. For the repeater group, the "history sheet" made out at the time of the last contact was secured. Then, as in the predictive study, tabulations were prepared and tested for statistical significance by the chi-square technique.⁷

The size of the sample used in the 1948 series of data was larger than that in the predictive study based on 1946 data. In all, there were records for 3,870 boys, of whom 1,844 were repeaters and 2,026 were one-time offenders. Some of the 1,844 repeaters had their first contact with the police in 1946; some before that date; and others in 1947 or 1948. Regardless of the date of their first police contact, for purposes of comparison with the one-time offenders, the last history sheet made out for 1948 was obtained and the data on it used in the tabulations.

It should be made clear that these two series of data deal with substantially different populations. Therefore, there can be no direct comparisons. Also, the populations are of different size. Therefore, the fact that the chi-square test proves a table to be statistically significant in the 1948 series,

although the comparable table in the predictive study was inconclusive, is meaningless without further analysis of the data. The chi-square total increases in direct proportion to the size of the population. A rather small rise in the chi-square total can throw a table from inconclusive to the significant category. To prove that there has been a genuine shift between the two series of data the most important percentage differences were tested for statistical significance by calculating the standard error of the difference and using this to obtain a critical ratio.

To avoid confusing the reader, we shall first report merely the tabulations showing differences which upon use of the chi-square test warranted rejection of the null hypothesis at the one per cent level of confidence.

As would be expected, the most striking fact about the 1948 series was that, of fifty comparable tabulations, only five failed to show differences significant at the one per cent level of confidence. The five items which were inconclusive were:

1. Mixture of business and residential land use in environs of boy's home.
2. Creed of boy.
3. Boy's preference in entertainment.
4. Period of day when parents were at home.
5. Excessive use of alcohol in the home.

The next most striking fact in a comparison of the 1946 and 1948 series is the relative stability of the facts as expressed in percentages. Also, of the nineteen items found predictive in the 1946 series, only one (boy's preference in entertainment) did not show in 1948 a difference significant at the one per cent level of confidence.

In the 1948 series, there were eight items relating to home conditions which had been inconclusive as predictors in 1946 but now held differences significant at the one per cent level of confidence. These eight were:

1. More repeaters complained about being picked on at home.
2. Fewer said they liked both parents.
3. More were found to have less recreational equipment than their playmates.
4. Fewer reported a sharing of activities in the home.
5. Fewer did chores around the house.
6. More kept all their money for their own use.

⁷ Statistical computations for this portion of the study were performed by James J. Balistrieri, a Research Associate at Wayne University.

7. More of the parents were openly punitive or rejective in their statements to the police.
8. More of the homes were broken.

Another cluster of statistically significant items relates to school. The full list is as follows:

1. Fewer liked their teachers.
2. Fewer liked their classmates.
3. More were getting poor grades.
4. More were in special schools.
5. More had quit school and found full-time work.

In all, five indices in the peer-group category involved differences significant at the one per cent level of confidence in 1948:

1. Fewer repeaters showed participation in organized games.
2. Fewer belonged to supervised, youth-serving organizations.
3. More belonged to organized gangs.
4. More had gone to the opposite extreme of being "lone wolves."
5. More belonged to gangs with a reputation for being unruly or engaging in organized theft.

Initially it had been expected that the fact of a boy's having failed to respond to their efforts would have soured the judgment of the police officers who had dealt with him. A halo effect in reverse could therefore be expected to color those items on which the most subjective ratings had been recorded. There was some evidence of this on the officers' judgments of the boy's attitude toward them; 48.4 per cent of the future repeaters in 1946 had been called "honest," but only 41.8 per cent of the confirmed repeaters in 1948.

In addition to the items reported above, there were eight which achieved statistical significance in the 1948 series but which fell into no clear cluster. For the sake of completeness they are listed below:

1. More of the repeaters came from homes with four or more brothers.
2. More of their parents were rated uncooperative by the police.
3. More were reported to be small for their age.
4. Fewer expressed friendliness toward adult neighbors.

5. Fewer repeaters lived in single-family homes.
6. More had both parents unemployed.
7. Fewer had recently moved to Detroit.
8. More were rated preadolescent.

The next question to be asked is which of these changes represent true shifts and which were merely a consequence of the fact that with a larger population the chi-square test confirms statistical significance in percentage differences which are inconclusive in a smaller population. To check on this possibility, the percentages in various categories for the 1946 and the 1948 repeaters were compared. The standard error of the difference was computed. Where the observed difference was three times its standard error, a genuine change could be considered verified.

When this test was used, it became apparent that the two sets of data were remarkably similar. Shifts in percentages were small, undramatic, and statistically inconclusive as far as social setting and home conditions are concerned. For instance, in 1946, 94.0 per cent of the future repeaters claimed they had been picked on at home; in 1948, the confirmed repeaters furnished 95.8 per cent giving the same complaint.

In two of the clusters, however, statistically reliable changes had taken place. These showed in items relating to school and to peer-group relationships. To illustrate, in the 1946 predictive study, 54.6 per cent of the future repeaters had expressed favorable attitudes toward school; in the 1948 group, this had dropped to 50.6 per cent. In the 1946 study, 16.4 per cent of the future repeaters had belonged to some adult-supervised youth group; among the confirmed repeaters in 1948, this had dropped to 13.5 per cent. Similar differences in attitudes toward police have already been mentioned.

What interpretations are to be placed on the above findings? It would appear that the bad family and neighborhood conditions which are predictive of repeating do not change for the worse as a boy becomes a habitual offender. This, of course, makes sense. The progress toward greater delinquency of a youngster does not change the socio-economic or sub-cultural forces which surround him. It is conceivable that as a boy gets in trouble over and over again, his

misconduct could place a strain on family ties, but if so, the effect is not great enough to give clear statistical signs.

If differences between future repeaters and confirmed repeaters are an indication of change, however, there is a shift in the way the boy is treated by the schools and the police. His grades apparently drop as he is more visibly delinquent, and he is likely to be shunted into special schools for behavior problem children. As a consequence he tends to be increasingly discontented with school. Also, the police apparently are more inclined to be suspicious of him. Both of these trends are almost inevitable social consequences of frequently getting in trouble. Whether or not they also raise the likelihood of further repeating is a possibility which merits further investigation.

Related to the above trends is the shift away from adult-supervised youth groups and toward either gang activity or its opposite extreme, isolation. Here, again, the find-

ing should be no surprise. The leaders of youth groups are understandably impatient with "trouble-makers" and may encourage them to leave organizations. Meanwhile, as a youngster gets in trouble frequently he may get to know more boys with a similar propensity to mischief. If he goes to a special school and spends time in detention his social contacts may become specialized. In many neighborhoods he may be isolated as less delinquent youngsters steer clear of him and of the trouble associated with him.

In summary, then, a comparison between a predictive study of juvenile repeating and a retrospective study of the same phenomenon would indicate that home and neighborhood conditions alter only slightly, but school, police, and peer-group relations become worse. In this way, the repeater is propelled toward more serious delinquency not only by the forces which started him on his way but also by more intensified pressures in other areas of his life.

COURTSHIP VALUES IN A YOUTH SAMPLE

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ALTHOUGH a broad literature exists regarding courtship in American society, little rigorous research has been conducted along this line. One important though relatively unstudied sphere has to do with courtship values.¹ Specifically, what traits are sought in a "date"? Does an individual's group statuses affect his values in this regard?

The present study investigates courtship values among three hundred and sixty-eight

unmarried students at the University of Colorado in the academic year 1950-51. Anonymous questionnaires were distributed during the class period to six lower division, two upper division, and one graduate class; to one fraternity; and to a group of graduate students who completed it independently. It was proved feasible to analyze these as one

Waller (revised by Reuben Hill), *The Family*, New York: Dryden Press, 1951, p. 156; Rex A. Skidmore and Anthon S. Cannon, *Building Your Marriage*, New York: Harper and Bros., 1951, pp. 83-84; Harold T. Christensen, "Dating Behavior as Evaluated by High-School Students," *American Journal of Sociology*, 57 (May, 1952), pp. 580-586; William M. Smith, Jr., "Rating and Dating: A Re-Study," *Marriage and Family Living*, 14 (November, 1952), pp. 312-316.

Most of the research on this subject has concentrated on the variable of sex differences in their relation to expressed preferences in date-choice. The present study will attempt a more comprehensive analysis.

¹ Among the studies in this specific field are: William G. Mather, "Courtship Ideals of High-School Youth," *Sociology and Social Research*, 19 (November, 1934), pp. 167-171; Willard Waller, "The Rating and Dating Complex," *American Sociological Review*, 2 (October, 1937), pp. 727-734; Rayanne D. Cupps and Norman S. Hayner, "Dating at the University of Washington," *Marriage and Family Living*, 9 (May, 1947), pp. 30-31; Harold T. Christensen, *Marriage Analysis*, New York: Ronald Press, 1950, pp. 212-214; Willard

large sample. Without specific design in this regard, the sample is completely Caucasian.

The questionnaire asked the student to specify, using his own words, the trait, quality, or characteristic he most desired in a "date."² This procedure of having the respondent indicate values in his own words involves greater difficulty in later organization of data than occurs when a prepared list of traits is presented for the person to rate in order of importance, a technique followed by many of the studies in this field. It was felt, however, that this present procedure would yield a more comprehensive and realistic picture of the individual's actual values.

In addition to the basic question on date-preference, information was also requested regarding age, sex, year in college, undergraduate grade average, fraternity affiliation, size of home community, family income (family of orientation), and extent of self-support.

FINDINGS

On the basis of the specific traits mentioned by the respondents, the authors established the following categories of traits:³

² It is interesting that even when an anonymous questionnaire is used and students are urged to be completely frank in specifying whatever traits they *really* seek, there may still be a tendency for the student to conform to the proprieties, to what he feels he *should* seek, or to what he feels the investigator would like to discover or expects to discover. For example, very few of the male students (and none of the female students) specified such traits as "sexy", "affectionate", or more earthy terms. From observation and interview, however, the authors are led to believe that sex experience is one of the prime dating-values and interests of male college youth.

On the other hand, it may be that the students answered quite truthfully and that therefore those critics condemning American youth for "low sex morals" are unjustified in their accusation. The sex-orientation of dating youth may be simply empty verbalizing to impress friends, to live up to an expected role. Actually it could be quite absent from the real value-motivations in dating behavior, as our findings seem to indicate.

It should be pointed out in this connection that socio-economically our sample is largely middle-class (See Table I concerning Family Income), and likely to share the inhibitions of this segment of society. A different situation may well exist for extremely upper or lower-class youth.

³ These categories were set up by the authors

The high importance placed upon traits relating to personal companionability can be noted above. Insofar as companionability is considered by most writers in the field to be an important positive factor in marital compatibility, our respondents demonstrate courtship values consistent with mature, realistic marriage-values. The traits they want in a *date* are those which also characterize a good *mate*.

This finding is of especial importance in view of the fact that many writers have claimed the opposite, that the artificial, commercialized culture-complex surrounding courtship in America sets up frivolous dating-ideals which are directly opposed to sound mate-choice and successful marriage. They say that our courtship pattern places prime importance on dancing ability, glamor and similar trivia. As a result, they claim, many persons carry these frothy courtship-ideals over into their mate-choice, only to learn to their later sorrow that the fancy dancer and glamorous party-girl are poor bets in the marriage marathon which requires more solid, enduring bonds. Our data reveal no such tendency. On the contrary, the heavy emphasis of our respondents on personal companionability indicates realism and maturity.

The next step was to discern through chi-square analysis whether statistically significant differences in date-preference existed

after the data were collected and include the following traits, specified by the respondents in their own words: COMPANIONABILITY—"congenial", "friendly", "pleasing personality", etc.; DESIRABLE PHYSICAL APPEARANCE—"good looks", "neat", "attractiveness", etc.; SOCIAL GRACES—"polite", "good manners", etc.; INTELLIGENCE AND EDUCATION—"intelligence", common sense", "education", etc.; MISCELLANEOUS (in declining frequency)—"good character", "sense of humor", "maturity", "considerate", "common interests", "good social background", "lightheartedness", etc.

Category	Number of Frequencies in Category	Per cent
Companionability	163	44.2
Desirable Physical		
Appearance	44	12.0
Social Graces	39	10.6
Intelligence and		
Education	30	8.2
Miscellaneous	92	25.0
Totals	368	100.0

between students of different age, sex, year in college, size of home community, undergraduate grade average, family income (family of orientation), fraternity affiliation, and extent of self-support. The data are given in Table 1.

Sex Differences. Sex variability in court-

ship values has received greatest attention in the literature. Our findings indicate that the apparent differences in date-preference between males and females which are revealed in Table 1 are statistically meaningful. The chi-square value is 21.78, which far exceeds the one per cent level of significance.

TABLE 1. GROUP DIFFERENCES IN DATE PREFERENCE, BY SELECTED VARIABLES *

	Date Preference											
	Companion-ability		Desirable Physical Appearance		Social Graces		Intell. and Educ.		Misc.		Totals	
	Num-ber	Per Cent	Num-ber	Per Cent	Num-ber	Per Cent	Num-ber	Per Cent	Num-ber	Per Cent	Num-ber	Per Cent
<i>Sex</i>												
Males	67	43.5	28	18.2	7	4.6	17	11.0	35	22.7	154	100.0
Females	96	44.8	16	7.5	32	15.0	13	6.1	57	26.6	214	100.0
Totals	163	44.3	44	12.0	39	10.5	30	8.2	92	25.0	368	100.0
<i>Age</i>												
17-18 yrs.	30	50.8	7	11.9	11	18.6	3	5.1	8	13.6	59	100.0
19-20 yrs.	77	53.5	12	8.3	13	9.0	10	6.9	32	22.3	144	100.0
21-22 yrs.	27	44.3	9	14.8	5	8.2	4	6.5	16	26.2	61	100.0
23 yrs. and over	29	27.9	16	15.4	10	9.6	13	12.5	36	34.6	104	100.0
Totals	163	44.3	44	12.0	39	10.5	30	8.2	92	25.0	368	100.0
<i>Yr. in College</i>												
Fr.	26	48.2	10	18.5	10	18.5	2	3.7	6	11.1	54	100.0
Soph.	51	51.5	8	8.1	13	13.1	7	7.1	20	20.2	99	100.0
Jr.	38	43.7	12	13.8	7	8.1	5	5.8	25	28.6	87	100.0
Sr.	28	41.8	9	13.4	6	9.0	5	7.5	19	28.3	67	100.0
Grad.	19	31.7	5	8.3	3	5.0	11	18.3	22	36.7	60	100.0
Totals	162	44.1	44	12.0	39	10.6	30	8.2	92	25.1	367	100.0
<i>Community Size</i>												
Farm, village, and city below 5,000	36	46.8	9	11.6	5	6.5	8	10.4	19	24.7	77	100.0
City 5,000-24,999	41	43.1	9	9.5	13	13.7	10	10.5	22	23.2	95	100.0
City 25,000-99,999	32	43.8	8	11.0	12	16.4	3	4.1	18	24.7	73	100.0
City 100,000 and over	54	44.2	18	14.8	9	7.4	9	7.4	32	26.2	122	100.0
Totals	163	44.0	44	12.1	39	10.6	30	8.2	91	25.1	367	100.0
<i>Grade Av.</i>												
A and B	71	41.5	18	10.5	11	6.4	19	11.2	52	30.4	171	100.0
C and D	91	47.2	26	13.5	28	14.5	9	4.6	39	20.2	193	100.0
Totals	162	44.5	44	12.1	39	10.7	28	7.7	91	25.0	364	100.0
<i>Family Income</i>												
Below \$4,000	34	42.0	10	12.4	9	11.0	5	6.2	23	28.4	81	100.0
\$4,000-5,999	40	40.8	16	16.3	10	10.2	7	7.2	25	25.5	98	100.0
\$6,000-9,999	28	43.8	9	14.0	4	6.2	6	9.4	17	26.6	64	100.0
\$10,000 and over	52	53.1	7	7.1	8	8.2	12	12.2	19	19.4	98	100.0
Totals	154	45.2	42	12.3	31	9.1	30	8.8	84	24.6	341	100.0
<i>Fraternity Affiliation</i>												
Fraternity or Sorority	105	49.0	26	12.2	24	11.2	18	8.4	41	19.2	214	100.0
Non-Frat. or Soror.	58	37.7	18	11.7	15	9.7	12	7.8	51	33.1	154	100.0
Totals	163	44.3	44	12.0	39	10.5	30	8.2	92	25.0	368	100.0
<i>Extent of Self-support</i>												
Not at all	74	50.0	15	10.1	19	12.8	9	6.1	31	21.0	148	100.0
Partially	60	44.4	14	10.4	17	12.6	12	8.9	32	23.7	135	100.0
Wholly	28	33.3	15	17.9	3	3.6	9	10.7	29	34.5	84	100.0
Totals	162	44.0	44	12.1	39	10.6	30	8.2	92	25.1	367	100.0

* The various totals in this table frequently differ, as not all students gave information in every case.

Most responsible for this contrast are the extremely meager emphasis placed on male "Desirable Physical Appearance" by the girls, and their heavy stressing of traits in the "Social Graces" category. Similarly responsible are the boys' exceptional emphasis on female "Desirable Physical Appearance" and their relative lack of attention to "Social Graces."

These findings are in general agreement with the normal cultural expectation regarding male and female values. Our society stresses female physical appearance more than male, and women in America tend to be more concerned than men with social forms and etiquette. Perhaps the coed also feels that the gentleman with "Social Graces" will react more graciously to the answer "no."

It is interesting that men place greater importance than women do on traits relating to "Intelligence and Education." The "dumb" date seems to be tolerated less by men than by women. Men also agree more fully on what they want in a date; women vary more widely in their specification of desirable traits. The greater modality of the male in this regard is indicated by a smaller representation of males than females in the category "Miscellaneous." This finding is interesting in view of opposite claims of generally greater variability and divergence from the norm, in characteristics and behavior, for the male.⁴ Essentially, however, Table 1 reveals that men and women seek approximately the same traits in a date, although they place varying importance upon them.

Age Differences. Students of different age vary markedly in their date-preferences. The chi-square value exceeds the one per cent level of significance, indicating that these differences between age-groups are statistically highly meaningful.

Of prime importance in this regard is the very low interest in "Companionability" by the older respondents (23 years and over), a finding which merits further study as it comes as something of a sociological surprise, and these persons' emphasis on the

great variety of traits within the "Miscellaneous" category. Indeed, a direct relationship appears between degree of norm-deviation in date-preference (as shown by a heavy representation in the "Miscellaneous" category) and age. We may draw the tentative generalization that the older the individual, the greater the variety of traits he seeks in a date. Younger persons tend to agree more fully on these values.

One may also note the high degree of importance placed by the 17-18 year group on "Social Graces," and the stressing of "Intelligence and Education" by the older group (23 years and over). The 17-18 year contingent seems to be swayed by values relating to "manners" and "politeness," while older persons look for more basic ones such as "intelligence."

Highly interesting is the greater emphasis on "Desirable Physical Appearance" by the older groups. The older person, perhaps responding to unconscious feelings of evanescent physical attractiveness, appears more beauty-motivated in courtship than does his junior.

Year in College. Chi-square analysis indicates that differences in date-preference between freshmen, sophomores, juniors, seniors, and graduate students, revealed in Table 1, are significant statistically at the five per cent level. Of great importance in this regard is the extreme emphasis on "Intelligence and Education" by the graduate students and the relative lack of interest in these traits by freshmen. This finding fits into a logical hypothesis that graduate students, a highly selective group intellectually and educationally, seek dates with individuals of high education and intelligence. These persons of high education and intelligence are persons like themselves. Here we have what can be called a *principle of assortative dating*: in courtship, people seek dates with persons like themselves.

Graduate students are the most varied in date-preference, as indicated by their specifying "Miscellaneous" traits to an unusually great extent. Probably related to the age factor⁵ discussed above, it appears from Table 1 that the higher the individual's edu-

⁴ William I. Thomas, *Sex and Society*, Chicago: University of Chicago Press, 1907, p. 51; Hans von Hentig, *Crime: Causes and Conditions*, New York: McGraw-Hill Book Co., Inc., 1947, p. 103.

⁵ Perhaps also related to the age factor is the graduate student's unusually meager interest in "Companionability."

cational attainment, the less conventional and norm-centered are his date-preferences. This may be a part of the over-all non-conventionality in values which, in the opinion of some analysts of the social scene, characterizes the intellectual.

The stressing of "Social Graces" by the freshman, and the graduate student's lack of interest in this category, are also shown in Table 1. This probably ties in somewhat with the age factor. Perhaps freshmen (clustered in the 17-18 year group), and coeds particularly, are a little over-awed by the large number of new faces in the somewhat frightening new environment of relative independence and anonymity that the university affords. They want to be certain that their dates (often persons they know only slightly) will be polite to them. They may be concerned about their own manners, too, and reflect this anxiety in their courtship values as applied to other persons.

The older, graduate student, on the other hand, is not apt to be anxiety-ridden concerning "Social Graces"—his own or those of his prospective date. He probably takes "Social Graces" more or less for granted. They have disappeared from the forefront of his consciousness so that they are no longer a primary factor in his date-choice.

At any event, there appears a general, inverse relationship between extent of education and emphasis on "Social Graces." The higher the person's educational level, the lesser the prominence of "Social Graces" as a dating-value. Although Emily Post may resent the finding, it appears that the highly educated person places only minor importance on etiquette as such. Perhaps it is subsumed in this person's specification of traits relating to "Intelligence and Education."

Grade Average. Undergraduate grade average was found to be of distinct statistical significance at the one per cent level. We may note in Table 1 that the better students ("A" and "B") emphasize "Intelligence and Education" in their choice of date, and place little importance on "Social Graces." This situation is reversed in the case of the poorer students ("C" and "D"), who stress "Social Graces" and neglect specifying traits which relate to "Intelligence and Education."

Once again, a *principle of assortative dating* seems operative. The more intelligent students (insofar as grades are an indication of intelligence) tend to prefer dates of similar intellectual status. "A" and "B" students, for whom intellectual and academic achievement is a value-motivation, express this same value in their choice of dates. These persons, like the persons of lengthy education discussed in the previous section, also appear to lay little stress on social forms and patterns of etiquette ("Social Graces") which are a distinct courtship value to the student of poorer academic performance.

It is likely that the poorer student's frequent sense of intellectual inadequacy makes him avoid dating persons of high intelligence, education, or scholarship, and that he prefers those whose intellectual attainments are comparable to his own. This fits the *principle of assortative dating*, and could explain the poorer student's lack of interest in "Intelligence and Education" in date choice.

The pronounced conventionality in date-preference among the "C" and "D" students, indicated by their relatively small choice of traits in the "Miscellaneous" category (Table 1), is likewise of interest. The brighter student ("A" and "B") seems to be less modal, more versatile in the qualities he seeks in a date. The more intelligent the person, it appears, the greater the range in his interests and values, and the less conventional will he be in these respects. In view of increasing conformity-pressures in American society, is this situation apt to change as the intelligent person learns that conformity rather than deviation holds greatest survival-value?

Fraternity Affiliation. Date-preferences of fraternity-sorority members and non-members reveal fairly significant differences. The chi-square value exceeds the five per cent level of statistical significance.

Determining factors include the stressing of "Companionability" by fraternity and sorority members and its relative neglect by non-members, a finding which merits further study. Of greatest importance, however, is the extreme conventionality and modality of the fraternity and sorority group in date-choice, whereas non-members are much more varied in their preferences. This is indicated

by the relatively small fraternity-sorority percentage in the "Miscellaneous" category and the comparatively high percentage of non-fraternity students in this classification.

The finding bears an interesting implication concerning the personality type of fraternity and sorority members. It may mean that fraternities and sororities are selective of conventional personality types whose date-choices are similarly conventional and modal. Perhaps "going fraternity" is in itself a conventional course of behavior for middle-class college youth. "Different" students are probably less motivated to join, as well as being less "rushed" by the fraternities themselves who are not anxious to have "characters" as members.

Extent of Self-support. Differences in date-preference between students of varying degrees of self-support were found to be quite meaningful. The chi-square value falls between the one per cent and five per cent level of statistical significance.

Most responsible is the extremely low interest in "Social Graces" by the wholly self-supporting group. Is it that this group is most mature and realistic in date-choice, and thus is little concerned with what it may feel are "frill" traits relating to the "Social Graces" category?

Tentative generalizations may also be made that the more self-supporting the student, the less his emphasis on "Companionability" and "Social Graces," and the greater his preference for "Desirable Physical Appearance" and "Intelligence and Education."

It is especially apparent that the wholly self-supporting student (who is male in all our cases) is highly disposed towards a physically attractive, intelligent date. Probably he can not afford to date as regularly as the other groups, so seeks something "special" when he does. An element of status-striving may also enter. Insofar as the wholly self-supporting student is apt to be particularly money-conscious, and perhaps burdened with fears of financial inadequacy, he may consider a highly attractive date to be worth in display-value the money he spends upon her. Because of her bargaining position, her tastes are likely to be more ex-

pensive than those of her less-attractive sister who is in less demand.

By dating a campus "queen," the self-supporter can prove to himself and others that he has money, that he can afford to take her out in the style to which she is accustomed. It puts him on a par with the wealthy "Joe Colleges" who are her usual escorts.

Another significant finding relates to the "Miscellaneous" category. It appears that the more completely self-supporting the student, the greater his variability in date-preference. The wholly self-supporting student is extremely variable in this regard, frequently choosing traits outside the modal categories. This rather unique type of college student, the total self-supporter, seems also to be quite unique in his courtship values.

Community Size and Family Income. No statistically significant differences in date-preference are found to exist between students from varying sizes of home community, or between students of different family income. We may conclude that neither farm nor metropolis leaves a distinctive mark on the courtship values of our youth. Nor does the wealthy family indoctrinate courtship values different from those of the poorer family. Insofar as wealth is an index of class status in America, we may infer that class factors are insignificant in determining what a person prefers in a date.

SUMMARY

The present study has investigated courtship values as expressed through date-preferences. Significant differences in these preferences are found to exist between students of different age, sex, year in college, grade average, fraternity affiliation, and extent of self-support. No significant differences in courtship values were observed for students of differing family income and size of home community.

A *principle of assortative dating* is found. The individual tends to prefer that his "date" have characteristics similar to his own. The data refute the popular notion that "opposites attract."

STUDIES IN CHILD SPACING: III—PREMARITAL PREGNANCY AS A FACTOR IN DIVORCE *

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RESEARCH relevant to the question of whether sexual intimacies before marriage are related to the success or failure of marriage, is scarce and somewhat inconclusive. Studies reported by Hamilton,¹ Davis,² Terman,³ Locke,⁴ and Burgess & Wallin,⁵ all tend to support the hypothesis that marriage is more successful where there has been no premarital sexual intercourse. Yet, most of these authors express certain cautions and qualifications. Furthermore, the Burgess & Wallin data "do not support the theory that coitus before marriage has an adverse effect on the *sexual relationships* after marriage."⁶

It was for the purpose of throwing additional light on this problem that the present study was undertaken. Our focus has been upon the phenomenon of premarital pregnancy as a factor in divorce, viewing the latter as an index of marriage failure.

In an earlier article by the senior author, it was reported that premarital conception was involved in approximately twenty per cent of all first births within marriage, and that disproportionately higher percentages

were associated with the depression marriages of 1929-31, a young age at marriage, a non-religious wedding, and a laboring occupation.⁷ These generalizations were based upon analyses of official marriage and birth data for Tippecanoe County, Indiana, for the years 1919-1921, 1929-31, and 1939-41.⁸

However, these earlier data provided no basis for testing the possibility of a relationship between premarital pregnancy and the success or failure of marriage. Since that time, we have secured from the files of the County Clerk's office available data for every divorce occurring in Tippecanoe County from the beginning of 1919 through the end of 1952. Names from this list were then carefully checked against those of the earlier marriage and birth lists to determine which, from our original samples, were also divorce cases. In this manner we were able to assemble complete marriage, birth, and divorce information on 137 cases.⁹

Preliminary description of divorce data. Divorce (137 cases) constituted 8.95 per cent of the total sample (1531 cases).¹⁰ Undoubtedly, this percentage, as found, is lower than one might expect if he were considering all present and future divorces

* The two earlier articles under this same general title dealt with the spacing of the first child from marriage and the measurement of premarital pregnancy. They are to be found in *Social Forces*, 31 (May, 1953), pp. 346-351, and *American Sociological Review*, 18 (Feb., 1953), pp. 53-59, respectively.

¹ Gilbert V. Hamilton, *A Research in Marriage*, New York: Albert & Charles Boni Inc., 1929, pp. 393-95.

² Katharine B. Davis, *Factors in the Sex Life of Twenty-Two Hundred Women*, New York: Harper & Bros., 1929, p. 59.

³ Lewis M. Terman, *Psychological Factors in Marital Happiness*, New York: McGraw-Hill Book Co., 1938, pp. 324-25.

⁴ Harvey J. Locke, *Predicting Adjustment in Marriage*, New York: Henry Holt and Company, 1951, p. 156.

⁵ Ernest W. Burgess and Paul Wallin, *Engagement and Marriage*, Chicago: J. B. Lippincott Co., 1953, pp. 353-390.

⁶ *Ibid.*, p. 366. Italics supplied.

⁷ Harold T. Christensen, "Studies in Child Spacing: I—Premarital Pregnancy as Measured by the Spacing of the First Birth From Marriage", *American Sociological Review*, 18 (February 1953), pp. 53-59.

⁸ Tippecanoe County's population is fairly homogeneous as to race and nativity, with less than 1 per cent Negroes and about two per cent foreign born.

⁹ Help in recording and checking was given by Witold Krassowski, Sybille Fritzsche, Marilyn Swihart, Delores Aurenz, Nancy Falk, and Lenore Miller. The earlier marriage and birth data had been gathered entirely by Olive P. Bowden.

¹⁰ Since our present concern is with the relationship between divorce and the timing of conception, our overall sample is the 1531 marriages for which we found evidence of the birth of a first child. For further detail see Christensen, *loc. cit.*

within the sample. But, since most of these marriages are still in process, and since there is no good way of following our couples once they have left the county, such a figure is unobtainable.¹¹

So far as our data reveal, there has been a general increase in divorce rate over time, with the depression marriages showing the highest rate of all. Considering, for this comparison, only those divorces occurring within eleven years from marriage,¹² we found rates of 4.52, 7.10, and 5.35 for the 1919-21, 1929-31, and 1939-41 year groups respectively.

Positive relationships were found between divorce and the following factors: young age at marriage, wide age differences between the mates, urban residence, unskilled occupation, nonreligious wedding, and remarriage. Since these findings are somewhat peripheral to the present study, and are to be detailed in a later article based upon a larger sample, we content ourselves here merely to report them.

Testing the relationships. In Table 1 are presented distributions of total cases and divorce cases according to the time interval between marriage and birth of a child. The first two columns show cases in which there is a reasonable presumption of premarital pregnancy.¹³

¹¹ Our method has the limitation of missing those divorces which took place outside of Tippecanoe County and which occurred (or will still occur) in years later than the ones searched. Some estimation of this loss can be obtained by comparing all marriages with all divorces in Tippecanoe County during the nine sample years of our study; thus considered, divorces (1042) amounted to 21.72 per cent of the marriages (4798). This means that we have located less than half of all actual and potential divorces in our sample (assuming that divorce in a parent-group such as ours is in as high a proportion as with all marriages, an assumption which we do not believe, but which we grant here in the interest of being conservative). However, since there is no known bias affecting these omissions, we feel justified in assuming that they will not substantially affect the comparisons of this paper.

¹² This was for the purpose of making the data comparable, since, for example one would expect to find a larger number of divorces in the 1919 marriage group during its 33 years of exposure than in the 1941 marriage group during its 11 years of exposure.

¹³ The normal period of uterogestation is 266 days. Premature birth would make a few post-

Divorce and interval to first birth are shown to be negatively related. Proportionately, there were over three times as many divorces in the shortest interval group as in the longest interval group, with the graduations in between running in a consistent direction. A chi square test revealed these differences to be significant beyond the .001 level of confidence.¹⁴

There are at least three important conclusions to be drawn from the above observation pertaining to our sample: (1) Disproportionately high divorce rates are found to be associated with the condition of premarital pregnancy. (2) Within the premarital pregnancy group, rates are higher (highest of any in the sample) for those who delay marriage until shortly before the expected birth. (3) Within the postmarital pregnancy group, rates are highest when conception takes place rather soon after marriage, lowest when it is delayed for several months or years.

What about duration of marriage as possibly affected by premarital pregnancy? Though our data are not entirely satisfactory with respect to this factor,¹⁵ they at least tentatively suggest a relationship; namely, the higher the incidence of premarital pregnancy, the shorter the duration of marriage. There were eleven divorces in our sample where marriage duration was less than two years. Of these, seven were premarital pregnancy cases and the remaining four involved early conception following marriage. Apparently, our premaritally pregnant couples get divorced, not only at a higher rate than others, but within a shorter time.

Controlling other variables. We have

marital pregnancy cases appear as premarital, just as late birth would make a few premarital pregnancy cases appear as postmarital, but it seems likely that these discrepancies would not be many. Cf. Christensen *loc. cit.*

¹⁴ It is possible that real differences are even greater than those found here due to differential migration away from the county. If, as is reasonable to suppose, disproportionately more of the premarital pregnancy cases migrated, in order to keep their problem secret, we would have missed disproportionately more of them in our search of the divorce records.

¹⁵ Some of the marriages of our sample, now intact, will later end in divorce. Without these, calculations on mean duration would be meaningless. Furthermore, our figures on cases with short duration are extremely small.

already reported association between premarital pregnancy on the one hand and such factors as young age, non-religious wedding, and a laboring occupation on the other. Similar relationships were found to exist between divorce and these very same factors. What we do not know at this point, therefore, is whether the disproportionately high divorce rate in the premarital pregnancy group is due to the reaction of the couples to premarital pregnancy, or to the prevalence in the premarital pregnancy group of other factors known to be associated with high divorce rate.

trolling the named variables. Divorce percentages now became 18.54 and 8.62 respectively for the experimental (premarital pregnancy) and control (postmarital pregnancy) groups. Thus, it would seem that, though the disproportionately high divorce rate is to be partially explained by the presence there of other divorce-favoring factors, a large part may also be accounted for by the fact of premarital pregnancy itself, or in other words, by the attitudes and reactions of couples with reference to this condition.

Summary and interpretations. This study

TABLE 1. DIVORCES DISTRIBUTED ACCORDING TO THE SPACING OF THE FIRST BIRTH FROM MARRIAGE

	Intervals Between Marriage and Birth of First Child (in days)					
Year Groups	0-139	140-265	266-391	392-531	532-1819	Total
1919-21						
Total Cases	25	80	151	88	143	487
Number of Divorces	3	11	17	5	6	42
1929-31						
Total Cases	23	77	85	48	119	352
Number of Divorces	5	15	9	8	10	47
1939-41						
Total Cases	23	119	139	102	309	692
Number of Divorces	6	13	8	4	17	48
All Sample Years						
Total Cases	71	276	375	238	571	1531
Number of Divorces	14	39	34	17	33	137
Per cent Divorces	19.72	14.13	9.07	7.14	5.78	8.95

To find an answer, we first eliminated from consideration all cases with time-intervals of 196-335 days—cases, in other words, where the placing of conception either before or after marriage might be held in some doubt. This left 178 cases where conception was rather definitely premarital and 989 where it was definitely postmarital. Divorce percentages within these two groups were found to be 18.54 and 6.27 respectively.

The second step was to match the two groups on the following factors: wife's age at marriage, age differences between the mates, place of residence, type of occupation, type of wedding, and number of times married.¹⁶ This, of course, had the effect of con-

has been based upon a sample drawn from Tippecanoe County, Indiana, using data from official marriage, birth and divorce records. Premarital pregnancy has been found to be associated with disproportionately high divorce rates. This was true even after the control, through matching, of interfering variables, and was especially true for cases in which couples waited until late pregnancy before marrying. There was also an indication that those premaritally pregnant couples who got divorced, did so sooner than others. Within marriage, early conceivers showed higher rates of divorce than did late conceivers. All of this is suggestive of the following typology:

(1) "Delayed marriage following pregnancy." This group is the most disposed toward divorce of any. For one thing, it is disproportionately loaded with divorce-dis-

¹⁶ Matching dichotomies were as follows: wife 20 or under, other; mates 4 or more years different in age, other; urban residence, other; unskilled occupation, other; religious ceremony, other; both first marriage, other.

posing factors—young age at marriage, wide age difference between the mates, urban residence, unskilled occupation, nonreligious wedding, and second or subsequent marriages. These factors do not account for all of the difference, however. In a culture such as ours, where premarital pregnancy is generally disapproved, one might expect those who "get caught" to be relatively more anxious and hence less adjustable. Furthermore, within this particular group where marriage is delayed until the last minute, so to speak, one might expect to find disproportionately more who are resentful over being forced into a relationship and determined to get out of it just as soon as they can.

(2) "Early marriage following pregnancy." This group has the second highest divorce rate in the sample. It, also, is rather heavily loaded with the divorce-disposing factors listed in the above paragraph. In addition, there would likely be some of the same anxieties surrounding a disapproved act as described above. There would be fewer forced marriages in this group, however, or, putting it another way, more of these had been planning marriage anyway and were in better positions to adjust to married life because of more favorable attitudes as well as previous preparation. Hence, the lower divorce rate than in group one.

(3) "Early pregnancy following marriage." This group has a lower divorce rate than those involving premarital pregnancy, but higher than those in which pregnancy is delayed for awhile after marriage. It is lower than the premarital pregnancy groups, both because of the presence of more favorable factors, and because of the relative absence of either anxiety or coercion. It is

higher than the delayed pregnancy group, probably because of the frustrations coming from unsuccessful birth control¹⁷ and from having the responsibilities of a child before the early marriage adjustments have become established.

(4) "Delayed pregnancy following marriage." This group has the lowest divorce rate of all. In addition to the advantages named for group three, it is largely free of the frustrations peculiar to that group. Here, pregnancy is kept within reasonable control, both as to the demands of the culture, and as to needs and desires of the couple.

Admittedly, some of these interpretations are mere speculations—hypotheses for future research. Beyond the statistical generalizations which we have been able to make for our relatively small group, lie other, more subtle, reasons for the behavior under study. While the explanations suggested above for our four types seem reasonable, they have not as yet been sufficiently tested. Especially needed are follow-up case studies and analyses of clinical data as applied to this problem in this particular sample. It would also be important to extend the testing of these relationships to other populations and the various subcultures of each.

¹⁷ Cf. Shirley and Thomas Poffenberger and Judson T. Landis, "Intent Toward Conception and the Pregnancy Experience," *American Sociological Review*, 17 (October 1952), pp. 616-620; also Harold T. Christensen and Robert E. Philbrick, "Family Size as a Factor in the Marital Adjustments of College Couples," *American Sociological Review*, 17 (June 1952), pp. 306-312. Poffenberger and Landis found larger proportions of unplanned pregnancies among couples who conceived early in marriage (p. 617). Christensen and Philbrick found lower marital adjustment scores for couples who had unplanned children (p. 309).

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A COMPARATIVE STUDY OF THE ROLE OF VALUES IN SOCIAL ACTION IN TWO SOUTHWESTERN COMMUNITIES *

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It is one of the central hypotheses of the Values Study Project that value-orientations play an important part in the shaping of social institutions and in influencing the forms of observed social action. By value-orientations are understood those views of the world, often implicitly held, which define the meaning of human life or the "life situation of man" and thereby provide the context in which day-to-day problems are solved.¹ The present article is an outgrowth of one phase of the field research carried out in western New Mexico. It presents the record of two communities composed of people with a similar cultural background and living in the same general ecological setting.

The responses of these two communities to similar problems were found to be quite different. Since the physical setting of the two villages is remarkably similar, the explanation for the differences was sought in the manner in which each group viewed the situation and the kind of social relationships and legitimate expectations which each felt appropriate in meeting situational challenges.

In this sphere of value-orientations a marked difference was found. Moreover, the differences in response to situation in the two cases were found to be related to the differences between the value-orientations central to these communities.

We do not deny the importance of situational factors. Nor do we intend to disparage the importance of historical convergence of value-orientations with concrete situations in explaining the centrality of some values as against others and in leading to the deep internalization of the values we discuss. But the importance of value-orientations as an element in understanding the situation of action is inescapably clear. All the elements of what Parsons has called the action frame of reference—the actors, the means and conditions which comprise the situation, and the value-orientations of the actors enter into the act.² The primacy of any one in any individual case does not permit generalization. Yet the present study testifies to the great importance of the third element—the value-orientations—in shaping the final action which ensues.

FOCUS OF THE INQUIRY

The inquiry is focused upon a comparison of the Mormon community of *Rimrock*³ with the Texan community of *Homestead*, both having populations of approximately 250 and both located (forty miles apart) on the

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¹ Clyde Kluckhohn, "Values and Value-Orientations in the Theory of Action: an Exploration in Definition and Classification," *Toward a General Theory of Action*, edited by Talcott Parsons and E. A. Shils, Cambridge: Harvard University Press, 1951, p. 410.

² Talcott Parsons, *The Structure of Social Action*, Glencoe: Free Press, 1949, pp. 43-86; *Essays in Sociological Theory*, Glencoe: Free Press, 1949, pp. 32-40; *The Social System*, Glencoe: Free Press, 1951, pp. 3-24.

³ "Rimrock" and "Homestead" are pseudonyms used to protect the anonymity of our informants.

southern portion of the Colorado Plateau in western New Mexico. The natural environmental setting is virtually the same for the two villages: the prevailing elevations stand at 7,000 feet; the landscapes are characterized by mesa and canyon country; the flora and fauna are typical of the Upper Sonoran Life Zone with stands of pinyon, juniper, sagebrush, and blue gramma grass and some intrusions of Ponderosa pine, Douglas fir, Englemann spruce and Gambel oak from a higher life zone; the region has a steppe climate with an average annual precipitation of 14 inches (which varies greatly from year to year) and with killing frosts occurring late in the spring and early in the autumn.⁴ The single important environmental difference between the two communities is that Rimrock is located near the base of a mountain range which has elevations rising to 9,000 feet, and a storage reservoir (fed by melting snow packs from these higher elevations) has made irrigation agriculture possible in Rimrock, while in Homestead there is only dry-land farming. Today both villages have subsistence patterns based upon combinations of farming (mainly irrigated crops of alfalfa and wheat in Rimrock, and dry-land crops of pinto beans in Homestead) and livestock raising (mainly Hereford beef cattle in both villages).

Rimrock was settled by Mormon missionaries in the 1870's as part of a larger project to plant settlements in the area of northern Arizona. Rimrock itself, unlike the Arizona sites, was established as a missionary outpost and the intention of the settlers was the conversion of the Indians, a task conceived in terms of the *Book of Mormon*, which defines the American Indian as "a remnant of Israel."

The early settlers were "called" by the Church, that is, they were selected and sent out by the Church authorities. The early years were exceedingly difficult and only the discipline of the Church and the loyalty of the settlers to its gospel kept them at the

task. Drought, crop diseases, and the breaking of the earth and rock dam which they had constructed for the storage of irrigation water added to their difficulties, as did the fact that they had merely squatted on the land and were forced to purchase it at an exorbitant price to avoid eviction. The purchase money was given by the Church authorities in Salt Lake City, who also supplied 5,000 pounds of seed wheat in another period of dearth. The original settlers were largely from northern Utah although there were also some converts from the southern states who had been involved in unsuccessful Arizona settlements a few years earlier.

As the emphasis shifted from missionary activities to farming, Rimrock developed into a not unusual Mormon village, despite its peripheral position to the rest of Mormonism. Irrigation farming was supplemented by cattle raising on the open range. In the early 1930's the Mormons began to buy range land, and Rimrock's economy shifted to a focus upon cattle raising. Today villagers own a total of 149 sections of range land and about four sections of irrigated or irrigable land devoted to gardens and some irrigated pastures in the immediate vicinity of the village. The family farm is still the basic economic unit, although partnerships formed upon a kinship basis and devoted to cattle raising have been important in raising the economic level of the village as a whole. In recent years some of the villagers—also on the basis of a kinship partnership—purchased the local trading post which is engaged in trading with the Indians as well as local village business. In addition to 12 family partnerships which own 111 sections of land, there is a village cooperative which owns 38 sections. Privately-owned commercial facilities in the village include two stores, a boarding house, two garages, a saddle and leather shop, and a small restaurant. With this economic variety there is considerable difference in the distribution of wealth.

The Church is the central core of the village and its complex hierarchical structure, including the auxiliary organizations which activate women, youth, and young children, involves a large portion of the villagers in active participation. The church structure is backed up and impenetrated by the kinship

⁴ For additional ecological details on the region see Evon Z. Vogt, *Navaho Veterans: A Study of Changing Values*, Peabody Museum of Harvard University, Papers, Vol. XLI, No. 1, 1951, pp. 11-12; and John Landgraf, *Land-Use in the Rimrock Area of New Mexico: An Anthropological Approach to Areal Study*, Peabody Museum of Harvard University, Papers, forthcoming, 1953.

⁵ L. City: ⁶ Se mittee, vestiga izens," 6, 1940

structure. Moreover, church organization and kinship not only unify Rimrock into a social unit, they also integrate it into the larger structure of the Mormon Church and relate it by affinity and consanguinity to the rest of Mormondom.

Rimrock has been less affected by secularization than most Mormon villages in Utah and is less assimilated into generalized American patterns.⁵ Its relative isolation has both kept such pressures from impinging upon it with full force and enhanced its formal and informal ties with the Church, preserving many of the characteristics of a Mormon village of a generation ago.

Homestead was settled by migrants from the South Plains area of western Texas and Oklahoma in the early 1930's. The migration represented a small aspect of that vast movement of people westward to California which was popularized in Steinbeck's *Grapes of Wrath* and which was the subject of investigation by many governmental agencies in the 1930's and 1940's.⁶ Instead of going on to California, these homesteaders settled in a number of semi-arid farming areas in northern and western New Mexico and proceeded to develop an economy centered around the production of pinto beans. The migration coincided with the period of national depression and was due in part to severe economic conditions on the South Plains which forced families to leave their Texas and Oklahoma communities, in part to the attraction of land available for homesteading which held out the promise of family-owned farms for families who had previously owned little or no land or who had lost their land during the depression. The land base controlled by the homesteaders comprises approximately 100 sections. Each farm unit is operated by a nuclear family; there are no partnerships. Farms now average two sections in size and are scattered as far as twenty miles from the crossroads center of the community which contains the two stores, the school, the post office, two garages, a filling station, a small

restaurant, a bean warehouse, a small bar, and two church buildings. Through the years, farming technology has shifted almost completely from horse-drawn implements to mechanized equipment.

With the hazardous farming conditions (periodic droughts and early killing frosts) out-migration from Homestead has been relatively high. A few of these families have gone on to California, but more of them have moved to irrigated farms in the middle Rio Grande Valley and entered an agricultural situation which in its physical environmental aspects is similar to the situation in the Mormon community of Rimrock.

THE MORMON CASE

In broad perspective these two villages present local variations of generalized American culture. They share the common American value-orientations which emphasize the importance of achievement and success, progress and optimism, and rational mastery over nature. In the Mormon case, these were taken over from the 19th century American milieu in western New York where the Church was founded, and reinterpreted in terms of an elaborate theological conception of the universe as a dynamic process in which God and men are active collaborators in an eternal progression to greater power through increasing mastery.⁷ The present life was and is conceived as a single episode in an infinity of work and mastery. The result was the heightening for the Mormons of convictions shared with most other Americans. Moreover, this conception was closely related to the belief in the reopening of divine revelation through the agency first of Joseph Smith, the original Mormon prophet, and later through the institutionalized channels of the Mormon Church. The Mormons conceived of themselves as a covenant people especially chosen for a divine task. This task was the building of the kingdom of God on earth and in this project—attempted four times unsuccessfully before the eventual migration to the west—much of

⁵ Lowry Nelson, *The Mormon Village*. Salt Lake City: University of Utah Press, 1952, pp. 275-85.

⁶ See especially the reports of the Tolan Committee, U. S. Congress, "House Committee to Investigate the Interstate Migration of Destitute Citizens," 76th Congress, 3rd Session, Volume 6, Part 6, 1940.

⁷ The data from Rimrock are based upon seven months field experience in the community during 1950-51. Additional data on this community will be provided in O'Dea's forthcoming monograph on *Mormon Values: The Significance of a Religious Outlook for Social Action*.

the religious and secular socialism of the early 19th century found a profound reflection. The Mormon prophet proposed the "Law of Consecration" in an attempt to reconcile private initiative with cooperative endeavor. Contention led to its abandonment in 1838 after some five years of unsuccessful experiment. Yet this withdrawal did not limit, but indeed rather enhanced, its future influence in Mormon settlement. The "Law of Consecration" was no longer interpreted as a blueprint prescribing social institutions of a definite sort, but its values lent a strong cooperative bias to much of later Mormon activity.⁸ In the context of the notion of peculiarity and reinforced by out-group antagonism and persecution, these values became deeply embedded in Mormon orientations. The preference for agriculture combined with an emphasis upon community and lay participation in church activities resulted in the formation of compact villages rather than isolated family farmsteads as the typical Mormon settlement pattern.⁹

While Rimrock and Homestead share most of the central value-orientations of general American culture, they differ significantly in the values governing social relationships. Rimrock, with a stress upon community cooperation, an ethnocentrism resulting from the notion of their own peculiarity, and a village pattern of settlement, is more like the other Mormon villages of the West than it is like Homestead.

The stress upon *community cooperation* in Rimrock contrasts markedly with the stress upon *individual independence* found in Homestead. This contrast is one of emphasis, for individual initiative is important in Rimrock, especially in family farming and cattle raising, whereas cooperative activity does occur in Homestead. In Rimrock, however, the expectations are such that one must show his fellows or at least convince himself that he has good cause for *not* committing his time and resources to community efforts

while in Homestead cooperative action takes place *only* after certainty has been reached that the claims of other individuals upon one's time and resources are legitimate.

Rimrock was a cooperative venture from the start, and very early the irrigation company, a mutual non-profit corporation chartered under state law, emerged from the early water association informally developed around—and in a sense within—the Church. In all situations which transcend the capacities of individual families or family combinations, Rimrock Mormons have recourse to cooperative techniques. Let us examine four examples.

The "tight" land situation. Rimrock Mormons, feeling themselves "gathered," dislike having to migrate to non-Mormon areas. However, after World War II the 32 returned veterans faced a choice between poverty and under-employment or leaving the community. This situation became the concern of the Church and was discussed in its upper lay priesthood bodies in the village. It was decided to buy land to enable the veterans to remain. The possibilities of land purchase in the area were almost nonexistent and it appeared that nothing could be done, when unexpectedly the opportunity to buy some 38 sections presented itself. At the time, the village did not have the needed 10,000 dollars for the down payment, so the sum was borrowed from the Cooperative Security Corporation, a Church Welfare Plan agency, and the land was purchased. The patterns revealed here—community concern over a community problem, and appeal to and reception of aid from the general authorities of the Church—are typically Mormon. However, Mormon cooperation did not end here. Instead of breaking up the purchased land into plots to be individually owned and farmed, the parcel was kept as a unit, and a cooperative Rimrock Land and Cattle Company was formed. The company copied and adapted the form of the mutual irrigation company. Shares were sold in the village, each member being limited to two. A quota of cattle per share per year to be run on the land and a quota of bulls relative to cows were established. The cattle are privately owned, but the land is owned and managed cooperatively. The calves are the property of the owners of the cows. The

⁸ The "Law of Consecration" became the basis of the Mormon pattern of cooperative activity also known as "The United Order of Enoch." Cf. Joseph A. Geddes, *The United Order Among the Mormons*, Salt Lake City: Deseret News Press, 1924; Edward J. Allen, *The Second United Order Among the Mormons*, New York: Columbia University Press, 1936.

⁹ Nelson, *op. cit.*, pp. 25-54.

project, which has not been limited to veterans, supplements other earnings sufficiently to keep most of the veterans in the village.

The graveling of the village streets. The streets of Rimrock were in bad repair in the fall of 1950. That summer a construction company had brought much large equipment into the area to build and gravel a section of a state highway which runs through the village. Before this company left, taking its equipment with it, villagers, again acting through the Church organization, decided that the village should avail itself of the opportunity and have the town's streets graveled. This was discussed in the Sunday priesthood meeting and announced at the Sunday sacrament meeting. A meeting was called for Monday evening, and each household was asked to send a representative. The meeting was well attended, and although not every family had a member present, practically all were represented at least by proxy. There was considerable discussion, and it was finally decided to pay 800 dollars for the job which meant a 20 dollar donation from each family. The local trader paid a larger amount, and, within a few days after the meeting, the total amount was collected. Only one villager raised objections to the proceedings. Although he was a man of importance locally, he was soon silenced by a much poorer man who invoked Mormon values of progress and cooperation and pledged to give 25 dollars which was 5 dollars above the norm.

The construction of a high school gymnasium. In 1951 a plan for the construction of a high school gymnasium was presented to the Rimrock villagers. Funds for materials and for certain skilled labor would be provided from state school appropriations, providing that the local residents would contribute the labor for construction. The plan was discussed in a Sunday priesthood meeting in the church, and later meetings were held both in the church and in the schoolhouse. Under the leadership of the principal of the school (who is also a member of the higher priesthood), arrangements were made whereby each able-bodied man in the community would either contribute at least 50 hours of labor or 50 dollars (the latter to be used to hire outside laborers) toward the

construction. The original blueprint was extended to include a row of classrooms for the high school around the large central gymnasium.

Work on the new building began in late 1951, continued through 1952, and is now (in 1953) nearing completion. The enterprise was not carried through without difficulties. A few families were sympathetic at first but failed to contribute full amounts of either labor or cash, and some were unsympathetic toward the operation from the start. The high school principal had to keep reminding the villagers about their pledges to support the enterprise. But in the end the project was successful, and it represented an important cooperative effort on the part of the majority.

The community dances. The Mormons have always considered dancing to be an important form of recreation—in fact a particularly Mormon form of recreation. Almost every Friday evening a dance is held in the village church house. These dances are family affairs and are opened and closed with prayer. They are part of the general Church recreation program and are paid for by what is called locally "the budget." The budget refers to the plan under which villagers pay 15 dollars per family per year to cover a large number of entertainments, all sponsored by the Church auxiliary organization for youth, the Young Men's Mutual Improvement Association, and the Young Women's Mutual Improvement Association. The budget payment admits all members of the family to such entertainments.

Observation of these dances over a six months period did not reveal any tension or fighting. Smoking and drinking are forbidden to loyal Mormons, and those who smoked did so outside and away from the building. At dances held in the local school there has been evidence of drinking, and at times fighting has resulted from the presence of non-villagers. But on the whole the Rimrock dances are peaceful family affairs.

Rimrock reveals itself responding to group problems *as a group*. The economic ethic set forth by Joseph Smith in the Law of Consecration is seen in the dual commitment to private individual initiative (family farms and family partnerships in business and agriculture) and to cooperative endeavor in

larger communal problems (irrigation company, land and cattle company, graveling the streets, and construction of school gymnasium). For the Mormons, cooperation has become second nature. It has become part of the institutionalized structure of expectations, reinforced by religious conviction and social control.

THE HOMESTEADER CASE

The value-stress upon individual independence of action has deep roots in the history of the homesteader group.¹⁰ The homesteaders were part of the westward migration from the hill country of the Southern Appalachians to the Panhandle country of Texas and Oklahoma and from there to the Southwest and California. Throughout their historical experience there has been an emphasis upon a rough and ready self-reliance and individualism, the Jacksonianism of the frontier West. The move to western New Mexico from the South Plains was made predominantly by isolated nuclear families, and Homestead became a community of scattered, individually-owned farmsteads—a geographical situation and a settlement pattern which reinforced the stress upon individualism.

Let us now examine the influence of this individualistic value-orientation upon a series of situations comparable to those that were described for Rimrock.

The "tight" land situation. In 1934 the Federal Security Administration, working in conjunction with the Land Use Division of the Department of Agriculture, proposed a "unit re-organization plan." This plan would have enabled the homesteaders to acquire additional tracts of land and permit them to run more livestock and hence depend less upon the more hazardous economic pursuit of dry-land pinto bean farming. It called for the use of government funds to purchase large ranches near the Homestead area which would be managed cooperatively by a board

of directors selected by the community. The scheme collapsed while it was still in the planning stages, because it was clear that each family expected to acquire its own private holdings on the range and that a cooperative would not work in Homestead.

The graveling of the village streets. During the winter of 1949-50 the construction company which was building the highway through Rimrock was also building a small section of highway north of Homestead. The construction company offered to gravel the streets of Homestead center if the residents who lived in the village would cooperatively contribute enough funds for the purpose. This community plan was rejected by the homesteaders, and an alternative plan was followed. Each of the operators of several of the service institutions—including the two stores, the bar, and the post office—independently hired the construction company truck drivers to haul a few loads of gravel to be placed in front of his own place of business, which still left the rest of the village streets a sea of mud in rainy weather.

The construction of a high school gymnasium. In 1950 the same plan for the construction of a new gymnasium was presented to the homesteaders as was presented to the Mormon village of Rimrock. As noted above, this plan was accepted by the community of Rimrock, and the new building is now nearing completion. But the plan was rejected by the residents of Homestead at a meeting in the summer of 1950, and there were long speeches to the effect that "I've got to look after my own farm and my own family first; I can't be up here in town building a gymnasium." Later in the summer additional funds were provided for labor; and with these funds adobe bricks were made, the foundation was dug, and construction was started—the homesteaders being willing to work on the gymnasium on a purely business basis at a dollar an hour. But as soon as the funds were exhausted, construction stopped. Today a partially completed gymnasium, and stacks of some 10,000 adobe bricks disintegrating slowly with the rains, stand as monuments to the individualism of the homesteaders.

The community dances. As in Rimrock, the village dances in Homestead are important focal points for community activity.

¹⁰ The data from Homestead are based upon a year's field work in the community during 1949-50. Additional data on this community will be provided in Vogt's forthcoming monograph on *The Homesteaders: A Study of Values in a Frontier Community*. See also Vogt, "Water Witching: An Interpretation of a Ritual Pattern in a Rural American Community," *Scientific Monthly*, LXXV (September, 1952).

These affairs take place several times a year in the schoolhouse and are always well-attended. But while the dances in Rimrock are well-coordinated activities which carry through the evening, the dances in Homestead often end when tensions between rival families result in fist-fights. And there is always the expectation in Homestead that a dance (or other cooperative activity such as a picnic or rodeo) may end at any moment and the level of activity reduced to the component nuclear families which form the only solid core of social organization within the community.

The individualistic value-orientation of the homesteaders also has important functional relationships to the religious organization of the community. With the exception of two men who are professed atheists, all of the homesteaders define themselves as Christians. But denominationalism is rife, there being ten different denominations represented in the village: Baptist, Presbyterian, Methodist, Nazarene, Campbellite, Holiness, 7th Day Adventist, Mormon, Catholic, and Present Day Disciples.

In the most general terms, this religious differentiation in Homestead can be interpreted as a function of the individualistic and factionalizing tendencies in the social system. In a culture with a value-stress upon independent individual action combined with a "freedom of religion" ideology, adhering to one's own denomination becomes an important means of expressing individualism and of focusing factional disputes around a doctrine and a concrete institutional framework. In turn, the doctrinal differences promote additional factionalizing tendencies, with the result that competing churches become the battleground for a cumulative and circularly reinforcing struggle between rival small factions within the community.¹¹

To sum up, we may say that the strong commitment to an individualistic value-orientation has resulted in a social system in

which inter-personal relations are strongly colored by a kind of factionalism and in which persons and groups become related to one another in a competitive, feuding relationship. The homesteaders do not live on their widely separated farms and ignore one another, as it might be possible to do. On the other hand, they do not cooperate in community affairs as closely as does a hive of bees. They interact, but a constant feuding tone permeates the economic, social and religious structure of the community.

RELATIONSHIP BETWEEN THE TWO COMMUNITIES

Although there is some trading in livestock, feed, and other crops, the most important contacts between the two communities are not economic but are social and recreational. The village baseball teams have scheduled games with one another for the past two decades, and there is almost always joint participation in the community dances and in the summer rodeos in the two communities. Despite Mormon objections to close associations with "gentiles," there is also considerable inter-dating between the two communities among the teen-age groups, and three intermarriages have taken place.

In general, the homesteaders envy and admire the Mormons' economic organization, their irrigated land, and more promising prospects for good crops each year. On the other hand, they regard the Mormons as cliquish and unfriendly and fail completely to understand why anyone "wants to live all bunched up the way the Mormons do." They feel that the Mormons are inbred and think they should be glad to get "new blood" from inter-marriages with homesteaders. They add, "That Mormon religion is something we can't understand at all." Finally, the homesteaders say that Mormons "used to have more than one wife, and some probably still do; they dance in the church, they're against liquor, coffee, and tobacco, and they always talk about Joseph Smith and the *Book of Mormon*."

The Mormons consider their own way of life distinctly superior to that of the homesteaders in every way. Some will admit that the homesteaders have the virtue of being more friendly and of "mixing more with

¹¹ This relationship between churches and factionalizing tendencies has also been observed by Bailey in his unpublished study of a community in west Texas, in the heart of the ancestral home region of the present residents of Homestead. Cf. Wilfrid C. Bailey, "A Study of a Texas Panhandle Community; A Preliminary Report on Cotton Center, Texas", Values Study Files, Harvard University.

others," and their efforts in the face of farming hazards are admired, but Homestead is generally regarded as a rough and in some ways immoral community, especially because of the drinking, smoking, and fighting (particularly at dances) that takes place. They also feel that Homestead is disorganized and that the churches are not doing what they should for the community. For the past few years they have been making regular missionary trips to Homestead, but to date they have made no conversions.

COMPARISONS AND CONCLUSIONS

In the case of Rimrock and Homestead, we are dealing with two communities which are comparable in population, in ecological setting, and which are variants of the same general culture. The two outstanding differences are: (a) irrigation versus dry-land farming and associated differences in settlement pattern, compact village versus isolated farmstead type;¹² (b) a value stress upon cooperative community action versus a stress upon individual action. The important question here involves the relationship (if any) between these two sets of variables. Is the cooperation in Rimrock directly a function of an irrigation agriculture situation with a compact village settlement pattern, the rugged individualism in Homestead, a function of a dry-land farming situation with a scattered settlement pattern? Or did these value-orientations arise out of earlier historical experience in each case, influence the types of communities which were established in western New Mexico, and later persist in the face of changed economic situations? We shall attempt to demonstrate that the second proposition is more in accord with the historical facts as we now know them.

Nelson has recently shown that the general pattern of the Mormon village is neither a direct function (in its beginnings) of the requirements of irrigation agriculture, nor of the need for protection against Indians on the frontier. Rather, the basic pattern was a social invention of the Mormons, motivated by a sense of urgent need to prepare a dwelling place for the "Savior" at "His Second Coming." The "Plat of the City of

Zion" was invented by Joseph Smith, Sidney Rigdon, and Frederick G. Williams in 1833 and has formed the basis for the laying out of most Mormon villages, even those established in the Middle West before the Mormons migrated to Utah.¹³

It is very clear that both the compact village pattern and the cooperative social arrangements centered around the church existed before the Mormons engaged in irrigation agriculture and had a strong influence upon the development of community structure not only in Utah but in the Mormon settlements like Rimrock on the periphery of the Mormon culture area. There is no objective reason in the Rimrock ecological and cultural setting (the local Navahos and Zunis did not pose a threat to pioneer settlements in the 1880's) why the Mormons could not have set up a community which conformed more to the isolated farmstead type with a greater stress upon individualistic social relations. Once the Mormon community was established, it is clear that the cooperation required by irrigation agriculture of the Mormon type and the general organization of the church strongly reinforced the value stress upon communal social action.

It is of further significance that as the population expanded and the Rimrock Mormons shifted from irrigation agricultural pursuits to dry-land ranching in the region outside of the Rimrock valley, the earlier cooperative patterns modeled on the mutual irrigation company were applied to the solution of economic problems that are identical to those faced by the Homesteaders. Moreover, in midwestern and eastern cities to which Mormons have recently moved, church wards have purchased and cooperatively worked church welfare plan farms.

In Homestead, on the other hand, our evidence indicates that the first settlers were drawn from a westward-moving population which stressed a frontier-type of self-reliance and individualism. They were searching for a place where each man could "own his own farm and be his own boss." Each family settled on its isolated homestead claim, and there emerged from the beginning an iso-

¹² Cf. Nelson, *op. cit.*, p. 4.

¹³ Nelson, *op. cit.*, pp. 28-38.

lated farmstead type of settlement pattern in which the nuclear family was the solidary unit. The service center which was built up later simply occupied lots that were sold to storekeepers, filling station operators, the bartender, and others, by the four families who owned the four sections which joined at a crossroads. Only two of these four family homes were located near the service center at the crossroads. The other two families continued to maintain their homes in other quarters of their sections and lived almost a mile from "town." In 1952 one of the former families built a new home located over a mile from the center of town, and commented that they had always looked forward to "getting out of town."

There is no objective reason in the Homestead ecological setting why there could not be more clustering of houses into a compact village and more community cooperation than actually exists. One would not expect those farmers whose farms are located 15 or 20 miles from the service center to live in "town" and travel out to work each day. But there is no reason why those families living within 2 or 3 miles of the village center could not live in town and work their fields from there. In typical Mormon villages a large percentage of the farms are located more than three miles from the farm homes. For example, in Rimrock over 31 per cent, in Escalante over 38 per cent, and in Ephriam over 30 per cent of the farms are located from three to eight or more miles from the center of the villages.¹⁴

It is clear that the homesteaders were operating with a set of individualistic property arrangements (drawn, of course, from our generalized American culture) and that their strong stress upon individualism led to a quite different utilization of these property patterns (than was the case with the Mormons) and to the establishment of a highly scattered type of community. Once Homestead was established, the individualism permitted by the scattered dry-land farming pattern, and encouraged by the emphasis upon the small nuclear family unit and upon multi-denominationalism in church affilia-

tion reacted on and strongly reinforced the value stress upon individual independence. It is evident that the homesteaders continue to prefer this way of life, as shown by their remarks concerning the "bunched up" character of a Mormon village and the fact that a number of families have recently moved "out of town" when they built new houses.

Of further interest is the fact that when homesteader families move to irrigated farms in the middle Rio Grande Valley, the stress upon individual action tends to persist strongly. They do not readily develop cooperative patterns to deal with this new setting which is similar to the situation in the irrigated valley of the Mormons at Rimrock. Indeed, one of the principal innovations they have been promoting in one region along the Rio Grande where they are replacing Spanish-Americans on the irrigated farming land is a system of meters on irrigation ditches. These meters will measure the water flowing into each individual farmer's ditches, and effectively eliminate the need for more highly organized cooperative arrangements for distributing the available supply of water.

In conclusion, we should like to reiterate that we are strongly cognizant of situational factors. If the Rimrock Mormons had not been able to settle in a valley which was watered by melting snow packs from a nearby mountain and which provided the possibilities for the construction of storage reservoir, they certainly could not have developed an irrigation agricultural system at all. In the case of Rimrock, however, the actual site of settlement was selected from among several possible sites in a larger situation. The selection was largely influenced by Mormon preconceptions of the type of village they wished to establish. In fact, Mormons chose the irrigable valleys throughout the inter-montane west. On the other hand, the physical environmental features for the development of irrigation were simply not present in the Homestead setting, and the people had no alternative to dry-land farming. There is no evidence to suggest that had they found an irrigable valley, they would have developed it along Mormon lines. In fact, the homesteaders' activities in the Rio Grande Valley suggest just the

¹⁴ See Nelson, *op. cit.*, pp. 99 and 144 for data on Escalante and Ephriam.

opposite. It is clear that the situational facts did not *determine* in any simple sense the contrasting community structures which emerged. Rather, the situations set certain limits, but within these limits contrasting value-orientations influenced the development of two quite different community types.

It would appear that solutions to problems of community settlement pattern and the type of concrete social action which ensues are set within a value framework which importantly influences the selections made with the range of possibilities existing within an objective situation.

FRIENDS, ENEMIES, AND THE POLITE FICTION

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SOCIAL interaction of any kind requires some degree of consensus. This is true only if the word consensus sheds its connotation of empathy, of emotional rapport, and is confined to meaning agreement on the terms of which interaction takes place. Consensus may thus be defined as the tacit delineation of mutually accepted norms of behavior. Since it takes two to make a quarrel, a quarrel requires consensus in this sense. The examination of certain situations in which consensus is purposefully manipulated may illuminate its significance in interaction; the primary object of this paper, however, is to relate the analysis of interaction to more general sociological categories, and thereby to develop further insights into the processes of social interaction.

Most of the commoner terms in the literature of sociology suffer from confusion and ambiguity, but none probably so much as "status" and "role". We shall, however, start from the definitions stated by Parsons: "A social system is a system of processes of interaction between actors. . . . Each individual actor is involved in a plurality of such interactive relationships. . . . The participation of an actor in a patterned interactive relationship . . . has two principal aspects. On the one hand, there is the positional aspect—that of where the actor in question is located in the social system relative to other actors. This is what we will call his *status*. . . . On the other hand, there is the processual aspect, that of what the actor does in his relations with others. . . . It is this which we shall call his *role*."¹ In this rendering, "status" has a locational, almost non-behavioral, reference; "role" denotes the be-

havioral aspect of participation in the social system. The attractiveness, and the dangers, in this use of the terms, lie in its derivation from a basic paradigm in intellectual manipulation—the graphical expression of a binomial. There is, of course, the convenient rag-bag of "personality" into which items not covered by the terms can be stuffed and packed off to the psychologists for sorting, but even so, there are specifically social elements in the individual's social behavior which are not comprehended within either term. Some of these elements are dealt with in this paper; meanwhile, the words "status" and "role" will be used to indicate the locational and action elements in social interaction.

The roles that an individual plays in different social situations may sometimes be present as possible alternatives in the same situation. A man may invite workmates or colleagues into his home and meet them in the same situation as that in which he enacts the role of husband and parent. The roles of husband and of parent may themselves overlap in this way in different situations within the home. At work, the member of the staff of a factory who is in a clique-relationship with some other members may find a similar overlapping, or juxtaposition, of the two roles. When the ambiguities in such situations become stressed, the fictive character of roles emerges into obviousness and a false social position has to be resolved by some

¹ Talcott Parsons, "The Social System," 1952, Tavistock Publications Ltd., p. 25. It will be noted that Parsons does at any rate drop the dubious element of expectation usually brought into the definition of "role."

² It is using the not in the or of the

declaration pointing to association with one of the groups—an affirmation of one status—and rejection of the other. But since such declarations carry with them a threat to a status in which the individual is involved, and which therefore constitutes a value to him, they are with rare exceptions covered by entry into a form of joking relationship and emerge as banter.

To use banter is to play at being hostile, distant, unfriendly, while intimating friendliness. It is a style of interaction used when two roles are presented to an individual and he decides to retain the status appropriate to both, while, as he must, acting out the role of only one. Banter, then, becomes a style for managing children taken by parents or teachers when other adults are present and forbid the adoption of a role attaching exclusively to their status as teacher or parent. It is frequent among adolescent boys and girls when they are together and there is a desire both to retain the security of the individual's status in his own peer-group and to assume a sexually attractive role towards the opposite sex. It is used by married people and intimate friends in argument, when each is concerned to maintain both the status of an intimate and the status of membership of a larger group whose prestige—as masculine, or as feminine, or as educationally, socially or economically powerful—weighs in the argument. In every case, the relationship with the group dominant at the occasion of interaction is retained; it is the other relationship which bears the episode of banter, as of less social significance at the time but nevertheless requiring safeguard for the future.

Status positions may be structured according to the esteem, and so the rights and privileges, accorded them in society; in effect, this is equivalent to structuring in terms of the security within the total environment offered by a status. But in a society in which status may be gained, and therefore may also be lost,² the occupancy of a status has constantly to be tested and proved. In social organizations which are instrumental in character—armies, factories

and working sub-communities and the like—status changes are frequent and status is the dominant and most clearly determined value. In such organizations small group membership is of importance to the individual, both in cliques to provide mutual validation for status and in cabals to extend each person's control over the status-gaining process. Situations are therefore constantly arising in which a clique-membership status is presented in interaction situations which also involve the status of membership in the organization as a whole. In certain areas of interaction, when clique and sub-community statuses come near to equivalence in importance, banter becomes the prevailing style—it is almost impossible to behave in any other way in messrooms and canteens. The following account illustrates the sort of occasion which gives rise to banter; it is representative of many such occasions in this organization. It may be remarked, in parenthesis, that considerably less security was attached to organizational status in this particular concern than in most, largely because of a policy of "allowing responsibility to grow with the person" and an associated policy of neither discharging nor downgrading; it was therefore necessary to check continually the security (i.e. the location in the esteem structure) of one's status and the adequacy of role-playing to find out whether the disguised adjustments to the organization which were the equivalent of bowler-hatting were not taking place around one. Membership of cliques and cabals (usually with the same people in both) therefore becomes of primary importance.

At the end of a foremen's meeting, the supervisor (Foreman A) of a department of skilled workers asked for a reconsideration of the earnings of two men who consistently failed to earn a bonus although their work was of a very high standard; they were both unmarried and were not worried about not making bonus, but he thought the special care they always gave jobs merited extra money, even though they were not liable to be met with demands to repeat jobs which had produced unsatisfactory articles. The chairman's view was that the provisions of the wage system met the case since the firm did not need articles produced to meet higher tolerances than were enough to meet design requirements, and that if

² It should be emphasized that we are here using the term purely in its "social location" sense, not in the sense of the "situations vacant" column or of the Census.

men were prepared to spend time doing this rather than in producing more in the same time, the factory lost as well as the men, and should certainly not have to pay more for doing so. There was by now fairly general discussion. At this point the supervisor received support from a departmental manager, one of a number of young men who had risen fairly rapidly in the firm. This man argued that the firm frequently needed work done requiring special care and that it should provide a financial reward to those who constantly employed it and who would be called upon on such occasions. The counter-argument, which was that such occasions were known to be allowed for by rate fixers, came from another member of the same clique (Dept. Manager B). Both became fairly involved in the positions they had taken up, and the discussion became warmer than any previous exchange in the meeting. The end came like this (as recorded on tape).

Foreman B.: You get this type of person in every department. You get the type that prefer to turn out a high class job and not bother about making bonus.

Chairman (to Foreman A.): Your men aren't complaining about this are they?

Foreman A.: No, only the rate fixer puts a time on the job, and they're taking time in excess of that.

Chairman: Well, they're below average then. And they're quite happy about not making bonus, and we're quite happy, at the moment, to have our machines run at a low utilization rate. If the time comes when we're short of machines, then we'll have to consider doing something about it.

Foreman A.: That's all I wanted to know—the general policy.

(Long pause—a feeling that the subject had not really been disposed of)

Dept. Manager B. (restating his previous argument): If we really had the class of work suitable for this type of man, then surely the men would earn bonus.

Dept. Manager A. (resuming argument): What do you mean by this class of work?

Dept. Manager B.: The high class work that these men seem to be prepared to do.

(Several voices)

Chairman (easing off): We're getting onto a rather thorny subject now. High class. . . .

Dept. Manager A. (raising voice: No, No. Supposing these two characters got married or came up against it—the sort of position most of us are in—their output would probably double at the same quality. Meantime we can't do much about it, but the existence of these two people in the factory has been quite justified.

Dept. Manager B. (grinning): Because of their low machine utilization?

In the case of this interchange, the socially dominant group was clearly the meeting, which represented indeed the factory organization itself for those present. It was necessary for departmental managers both to act out roles appropriate to their special status in it, which, as they were members of a young, rapidly promoted group, comprehended a special degree of involvement in the welfare of the firm and a capacity for ready judgment and apt verbalization. On the other hand, the intimate and valuable relationship within the clique had to be safeguarded. Short of prearranged rehearsal of serious argument, which too would have had the character of play, banter was the only way out.

The two-fold nature of the banter relationship seems to indicate that "status" is not entirely adequate as a description of a social location. It is implicit in the usage of the term that there should be incompatibility to some extent between one status and another, as there is between one role and another; if two roles are not incompatible, in any sense, (i.e., do not contain within their range of permitted behavior acts which would be inappropriate to the other) then they are, of course, one and the same. Here we have a type of situation in which a dual status is possible. The fact is that, in the interaction situation, a status is the "membership of a group and of a particular category" claimed by the individual and ad-

mitted by the group. The admission of the claim is conveyed by consensus, its rejection by the withholding of consensus.

In all societies, the joke is the short cut to consensus. And it is the characteristic double understanding of the joking relationship that permits of the maintenance of two status positions through the same unit of social action, through performance in the same "role." It allows consensus to exist on two planes, so to speak, when consensus, and the member relationship it subtends, is almost by definition an element exclusive to one relationship at a time; the friendly ridicule of banter is an act of overt exclusiveness which, by sharing the joke with the excluded "victim", includes him in a special relationship with the actor. The effect is to maintain undamaged the status pattern—the nexus of memberships—pertaining to an individual.

There are other occasions when what we may call a primary status membership of a socially dominant group is threatened by the simultaneous presentation of an alternative, secondary, status membership, which on its side is not valuable enough to be safeguarded. However, simple rejection of a relationship is damaging to the primary status, implying a disregard for values which is dangerous to the esteem structure in which the primary status is located. The style normally used in rejection is again one which encases the act in a form of joking relationship. To use irony is to play at being friendly—at maintaining a member-relationship—while intimating enmity, rejection. In this case, the element of reassurance in the joking relationship refers to the status occupied in the group more socially significant to the occasion—the joke is shared with them at the expense of the other. With both banter and irony the first object is the defence of the primary status against the threat of the simultaneous presence of another; to accept the secondary status is to abdicate from the other.

The child, ridiculously exaggerating the strength and prowess of another, establishes a joking relationship with his peers and safeguards his position in the esteem structure of the group when it is threatened by the secondary subordinate status offered

in the overweening demonstration or claims of the other. The force of the irony lies in the convention which disguises hostility in a style overtly connoting goodwill, helpfulness, friendliness.

A declaration of enmity is in fact a rejection of a secondary member-relationship to accept which would threaten the secure occupation of a primary membership. But the outright declaration of enmity would provoke a conflict which would, whatever the loss or gain which came out of it, damage the primary membership. It would damage it because consensus between an individual engaged in a conflict and those not so engaged would be impossible.³ The entry into irony, by establishing consensus along the line of the primary relationship through the shared joke, allows the rejection of the secondary relationship without danger to the primary. The ironic style comes a good deal into interactions engaging persons whose status is insecure or vague, and is operated especially by persons in motion through esteem systems. It may often be sequentially related to banter. There comes a period in the career of the social or occupational success when the status in a group or clique or cabal useful and significant at an earlier stage has to be safeguarded on occasions by banter, and a later period when it has to be rejected by irony. The behavior of successful Trade Union leaders and industrialists who become successful politicians may follow such a pattern as may that of women making successful marriages, children emerging from street play groups, schoolboys being made prefects, students being appointed to the University staff. It is displayed typically in some middle class treatment of Negroes and Jews, in the treatment of some adult children by parents and of some parents by their adult children. Fundamentally, its use is in those situations when primary status has to be protected without disrupting the social organization in which both primary and secondary statuses are involved. A second illustration, drawn from the same milieu as the first, may demonstrate more clearly

³ Although consensus may be replaced by loyalty, in which consensus itself has become institutionalized, and the relationship safeguarded.

the operation of this particular style of manipulating consensus.

The manager of a small branch factory of a large concern was at the head office talking over current difficulties of selling his particular product with the sales manager and the general manager. The branch had been through a difficult period of building up production, and the rather unexpected shrinkage of the market was causing its head some concern; the discussion was therefore being carried on seriously, that is, each assumed an equally serious attitude in advancing his views. The office was closed, and there was consequently less pressure for decisions as a result of this particular discussion, but there was obvious unity in the acceptance of the purpose of the discussion and of its importance. After some time, the chairman looked in. The chairman was a man of fairly wide industrial interests and with a growing public reputation. He listened in silence for a minute and then turned to the general manager and said "But, Frank, I am right, aren't I, in thinking that sales of this particular job don't matter so much? You were saying, weren't you, a couple of days ago, that you weren't worried about selling this particular job. We had plenty of use to make of the space if it was scaled down." After the embarrassed pause which this entry provoked, the chairman rephrased the statement, using his normal, rapid, rather emphatic delivery, but winking broadly at the branch manager and the sales manager. These two became involved in the amusement which continued through several variations of the same theme, to none of which, naturally enough, could the general manager find any sufficient response. In the end, the general manager entered the amusement and the situation dissolved. There was, of course, no attempt to reconcile the attitude of involvement displayed by the general manager in the earlier part of the discussion with the markedly dissociated opinion betrayed by the chairman.

The ironic elements in the situation were the sharing of the joke with the others present, including the observer, and the superficial attitude of association with the general manager in clarifying the situation. According to the present interpretation, the chairman was engaged in rejecting a secondary status implied in a high-up, policy-making, clique relationship between himself and a man in a subordinate position in the con-

cern, and asserting a primary status clear above all three other members of the organization present, a status, that is, in which association with the sales manager and a branch manager could be treated by him, on occasion, as equivalent or primary as well as secondary to that between himself and the general manager. Had he kept silent, he would have accepted the clique relationship implied in the secret knowledge.⁴

This sort of occasion occurs as one among many. To treat it in isolation is to give it, as far as the actors were concerned, an utterly disproportionate significance. Other occasions would, in fact, display quite other or even contrary significance for the status of the people involved. Interactions between members of an enduring community take on a regulatory or cybernetic character, especially when, as in an industrial concern, status changes are frequent.

Secondly, the occasion for the use of the safeguards operating through banter and irony appears only when two discrete status positions are presented to an individual simultaneously. It was perfectly possible for the clique members to occupy unreservedly and securely their status positions within the clique when interaction situations included only other members of the clique; it was similarly possible for them to act out the roles appropriate to their status within the larger groups of the organization in the absence of other members of the clique. And it is quite possible for no need for safeguarding to arise even though the clique and the major group is represented in the situation, provided that no threat to the primary status emerges from the situation.

We carry with us the capacity for acting out a number of roles, for occupying a variety of status positions. A function of that capacity is to keep the roles and status positions discrete—to act out a role in which we follow modes of behavior more or less inappropriate to other roles. The foregoing sections have reviewed the procedures usually followed when this inappropriateness becomes actually embarrassing. Normally, of course, roles are acted out in separation and without any of the embarrassment, social or moral, which may result from their juxtaposition.

⁴ See K. H. Wolff, "The Sociology of George Simmel," *The Free Press*, 1950, pp. 330-338.

position in the same interaction situation.

In terms of social structure, cities are arenas for status-gaining or status-changing activity. They can be so by virtue of the discreteness of social institutions in them, a discreteness which is demonstrated in the functional differentiation of urban areas and in the demarcation of clearly defined sectors in the life-space of the individual. Prestige or status can be improved much more easily in one sector than in all, in one institution than in all. Occupational promotions are worked out separately in terms of improvements in class status, in prestige within the clique and so on. The embarrassments usually attendant upon change of role which may have to be overcome by banter and irony are in fact mostly avoided by resigning from one milieu or group and gaining admission to another. Status gains, that is, are not usually registered within the same groups, but by movement between groups. Urban society, because of the discreteness and multiplicity of its institutions, provides perpetual opportunities of escape from the embarrassments of a new or challenged status.

For instance, in interviews with some fifteen members of the executive grades in a factory in a large town, it appeared that more than half had moved house within two years of obtaining their biggest rise. In most cases also, the rise in occupational status had itself involved a substantial lateral move in the organization, and in some, a move from one city to another.

In terms of a cultural tradition framed in terms of community status, this discreteness of status positions looks as if individuals in different status positions behaved as though they were different persons. This characteristic of urban society has given rise to a number of speculations about its schizophrenic nature. "As if" behavior in urban society is so functionally necessary that it has to be arranged for even when two different statuses are occupied in two groups with virtually the same membership. In those cases cited above in which there was still overlapping between, for example, neighbor group and work group, the shift in occupational status was treated by an "as if" arrangement until a move of residence could be made. As one man, who had risen

rapidly to the position of deputy departmental manager but who still lived in a housing estate near the works put it "Jock's all right in the cinema queue, but it's Mr. Cullen on the shop floor. If any of them call me 'Jock', then I know they're trying to start something."

The conditions affecting similar situations in small communities are different. There is no possibility of maintaining groups so discrete that status changes can be made in one sector and then be validated in others. Thus status gains in occupation, sport, political and church groups, in clubs and associations, and even through marriage tend to remain isolated. In contrast with the city factory executives, men who had risen to foreman, department manager and higher ranks in a small town factory all retained first-name relationships with the work people—not only those whom they had previously worked alongside, but newcomers also. The only cases in which promotion had been followed by a change in residence were those facilitated by the existence of a group of "firm's houses" reserved for managerial staff. It was noticeable that people at managerial level recruited directly from large cities tended to use, in their relationships with work people, the behavioral cues and status symbols current in urban society, and also to remain conscious of their stranger status for periods of up to twenty years residence in the town.

"As if" arrangements of another kind are therefore necessary in local communities, this time being directed against the acknowledgment of a particular status dissociated in rank from the community status—the position occupied in the esteem system of the whole community. In terms of the present analysis, this indicates that no break in membership is permitted; consensus has to be maintained in all interactions between members of the community. (Thus, while banter is a permitted—indeed a frequent—style, irony is not permitted, and when used isolates the actor, not the victim, from the primary group.)

During one period of field work in a factory in a small town, I had occasion to transcribe certain figures and other records kept in the office of a departmental manager in a factory. One afternoon, a conversation with the manager led to his talking about

the effects of promotion on the behavior of different men in the factory, and eventually, to something of a diatribe against one particular person who had moved up into an executive position the year before. He had tried to get support for his promotion from everybody, had blackened the man who was leaving and whose post he hoped to fill, had gone into local politics on the same side as the divisional manager, had displayed unpleasant anxiety when the time came for the decision. Now, despite his fulsome affability, he was unpopular with his colleagues, was looked on by those lower down as a talebearer, and so on. All this was delivered with gestures and emphasis distinctly more lively than previously in the conversation, which ended with this episode, both of us returning to separate desks. Later in the afternoon, he telephoned the man of whom he had been speaking; there was a question about the allocation of a morning's time put in by a shiftworker in one or the other of their departments. The whole matter, which could easily have been a matter of dispute, was handled with the greatest mateyness and ease; first names were used, there was appreciably no sense of effort in maintaining the demonstration of friendliness; there was no over-emphasis, nor, on the other hand, any discrepancy between facial expression and words or tone of voice; each other's account of the facts was fully accepted and agreement quickly reached.

Inside the space of one hour, my companion had displayed quite marked enmity and equally well marked friendship towards the same person. There was, as far as I could judge, no suspicion of awareness that there was any incompatibility between the two episodes—both were acted through as natural expressions of two distinct roles.

In addition to the safeguard styles of banter and irony, and the means of avoidance possible in urban society, there exist the devices of "as if" arrangements by which difficulties in interaction encountered by the discrepancy between status positions occupied by the same individual may be surmounted. "As if" arrangements may be subsumed under the general heading of polite fictions. The arrangements are in general directed toward the exclusion from the terms of interaction of any status occupied by a participant which is incompatible with the establishment of the consensus necessary for participant membership alongside the rest of the group.

Role displacement—the substitution of an entirely new, incompatible role for another—is as familiar a feature of the careers of most people as physiological change, and occurs as a well-marked aspect of the process of socialization. But what also occurs in socialization is a change in behavior within the same role. While expectations of behavior change with roles when the child moves out of his position in a family situation to the street group or the school or the extended family, expectations also change when the child reaches certain ages; actions greeted with the pleasant attention of laughter or affectionate remonstrance at eighteen months may arouse much less pleasant responses at three years, and violence at a later age. Socialization can indeed be seen as a process by which society continually raises standards on the role of the child in this way.

The shift of behavioral patterns within the same role is typical not only of the socialization process, but also of other constellations of institutionalized behavior. In Scotland, where Sunday is marked out with special behavior and symbols by a significantly large proportion of the population, role-behavior in the family, the extended family and in the residential sub-community, undergoes considerable change of a cyclical character. Other changes are also regularly observable at other times in the week—for instance, on Saturday afternoon, and at special periods in the year such as the summer holiday and festival days. It is not possible to regard changes of this sort as extensions of some role, since there is implicit in them the same inconsistency with the normal, everyday patterns of behavior. There is, in fact, the displacement of behavioral features which is normally associated with change of role.

In small communities, social situations of different kinds (at work, in the public house, in church, at sport, at home, in clubs) so often involve the same group membership that, as Kaufman has shown,⁵ status positions in each of them are merged in, and indistinguishable from, a generalized community status. Nevertheless, institutional behavior varies with each different situation

⁵ Kaufman, Harold F. "An Approach to the Study of Urban Stratification," *American Sociological Review*, 17 (August, 1952), p. 430.

encountered by the individual in a small community, and not merely with the overt purpose of the group.

In all situations which admit of typical, institutionalized, behavior, such behavior has to be validated consensually. Change from one form of behavior to another, whether through a cycle of status positions or through a cycle of institutions, denotes a change from one application of consensus to another. The establishment of consensus is mediated by the exchange of behavioral cues, sometimes obvious—as in the case of children when emerging from the school gates—sometimes extremely subtle—as in Quaker business meetings, in which unanimity has to be achieved and voting procedures are not admitted. What is conveyed *through* such behavioral cues is the new range of permitted behavior and its further modal definition through approval and disapproval. The outcome of the establishment of consensus is membership, not merely of the group involved in the interaction, but of a type, a category identified with some precision in the schema of the social system with which a person socialized into it operates. The congruence of such classificatory schemas in an interaction through consensus allocates a status to the participant individuals.

The classificatory schema is more refined and detailed in respect of behavioral categories lying close to the individual, that is, more familiar, than at the periphery. Social distance straightens behavior in interaction because the categories involved are peripheral to the individuals concerned, and are prescribed by a minimal range of norms. Interaction of this kind runs to stereotype behavior or more properly, cliché behavior. Foreign visitors to a country are often disconcerted to find not only that they are being treated as stereotype Englishmen, Americans, or Indians but that they are in fact behaving like stereotype Englishmen, Americans, or Indians. At the other extreme, interaction between persons socialized into the same classificatory schema either through membership in the same local community or through long intimacy is prescribed by a maximal range of norms and runs to spontaneous behavior.

Spontaneity in interaction springs from a consensus so comprehensive that the be-

havior possible in the circumstances is no less than what each socialized individual would condone or approve in himself. Thus spontaneous interaction is determined by the existence of a consensus applying to all norms of social behavior of the system into which the individuals concerned have been socialized.

Between spontaneous and cliché behavior in interaction lies an array of possible categories of action identified in common usage as "role-playing"; such categories are marked by a restrictive, delineating, quality, with criteria to distinguish appropriate from inappropriate behavior. The criteria are of two kinds. One applies to the place in the value system occupied by the institution in which the interaction situation occurs; the other applies to the places in the esteem system occupied by the participant actors. With the first kind, institutional behavior gains in complexity and in variability the higher in the scale of value it lies. One may, for example, compare the variety of forms of religious action in the Middle Ages, especially the inclusion of spontaneous activity in art forms of poetry, music, painting and dancing with the relatively specific nature of religious action now. The higher the place occupied by an institutional form, the greater the number of social norms relevant to behavior in situations occurring in the institution. As for the second criterion, it has been sufficiently established that the esteem system of a group, community, or society, is structured according to the social norms prevailing among their members. In G. C. Homan's words, the persons coming closest to achieving the group norms hold the highest social rank.⁶

The norms which apply to any particular interaction are a selection from the whole range applying to the whole socialized behavior common to the participants, the selection being determined by the institutional character of the interaction and by the rank of the participants. The way in which the norms are mediated into action—the way in which people make themselves aware of how to act in the situation—is by the operation of consensus. The classificatory schema referred to above may then be viewed as a

⁶ Homans, G. C. *The Human Group*, London, 1951, p. 179.

hierarchy of consensus subtending the norms of behavior into which individuals are socialized. Differences in the social behavior of a person can be regarded as differences in the *number* as well as the *kind* of norms involved in the situation. Cliché behavior involves fewest norms, requiring little consensus. Spontaneity involves most norms, requiring maximum consensus. Between these two extremes lies social action involving varying ranges of norms determined by the values accorded to the situation and the participants and requiring an appropriate degree of consensus.

The phenomenon of the "polite fiction" appears now as an intrinsic element in social behavior. Status exists as membership first of the group in which the position is occupied and secondly of a rank order in the esteem system; status is realized only in such membership. The prerequisite of membership is consensus extending through the group in interaction. Consensus is necessary at whatever normative level interaction occurs, subtending the range of norms to which behavior in it relates. In passing from one interaction to another, the individual moves from one status to another. This usually involves both a displacement of membership and a shift in the range of social norms engaged. Both changes must be validated by the establishment of consensus in the new situation as in the old. This is accomplished naturally and with complete freedom in most cases. But, as we have seen, occasions arise when the memberships involved in two separate, although proximate, situations overlap. There is then presented a dilemma situation of two possible ranges of norms of differing coverage, each with its appropriate level of consensus. Such accidents are quite frequent in most individual's lives. When two people with a fairly intimate relationship allowing for an approximation to spontaneous behavior are joined by a third acquaintance, the change to a new consensus subtending a more restricted range of norms is almost always automatic and unthinking. Not to execute the change, or to revert to the previous consensus and normative level is, in any society, bad manners, an affront to the newcomer which lies in rejecting the

status-membership claim implied in his presence. It may well be, of course, that experience of occasions on which such rejection has occurred accidentally or intentionally, or when cues have been misinterpreted as rejection, arouses some fleeting sense of artificiality in making adjustments of consensus in this way. More definitely, "as if" arrangements in which two status memberships are occupied on different occasions in a group with the same participant members induce such a sense. In military service, the dual nature of the relationship of a superior officer or N.C.O. with the group immediately under his command is given acknowledgment in the phrase "on parade, on parade; off parade, off parade".

Embarrassment arises through the failure to establish or maintain consensus about the range of social norms affecting behavior in an interaction. It is potential typically in situations in which two statuses are presented simultaneously. Two devices are commonly used to avoid embarrassment. In one kind of situation dealt with in this paper, the status alternative to that occupied by a membership of the group dominant in the interaction is lower in the esteem system operating for the group; the ironic style admits of rejection of the lower status while safeguarding consensus through a joking relationship with the dominant group and the primary status within it, a consensus which might be broken by a display of bad manners. In the other kind of situation reviewed, the secondary status presented is also rejected, but safeguarded for the future, again through the joking relationship. We have called the two styles irony and banter, with some sense of straining ordinary usage, because the meanings of the two words contain, without precision it is true, the implications of a consensus withheld from the recipient in the case of irony,⁷ and shared with him in the case of banter.

⁷ Fowler, in distinguishing the correct usages of humor, wit, sarcasm, irony, etc., presents them in an interesting table, in which the aim or motive of irony is identified as "exclusiveness," its province as "statement of facts," its method or means as "mystification" and the audience as "an inner circle." (H. W. Fowler, *A Dictionary of Modern English Usage*, Oxford: 1926, p. 241.)

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NOTES ON RESEARCH AND TEACHING



ECOLOGICAL REGRESSIONS AND BEHAVIOR OF INDIVIDUALS *

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In a very interesting paper¹ which was published in 1950, W. S. Robinson pointed out that ecological correlations cannot be used as substitutes for individual correlations. He stated the purpose of his paper will be accomplished "if it prevents the future computation of meaningless correlations and stimulates the study of similar problems with the use of meaningful correlations between the properties of individuals." Since 1950, many papers which base conclusions on ecological correlations have been published. The purpose of this note is to help clarify this problem. We shall emphasize the point that *in general* the study of the regression between ecological variables cannot be used as substitutes for the study of the behavior of individuals. We shall also point out that *in very special circumstances* the study of the regression between ecological variables may be used to make inferences concerning the behavior of individuals.

Why do sociologists study the regression between ecological variables? One answer is that they are interested in the questions which that kind of study can answer. For example, they may be interested in knowing whether or not there is a linear relation between the per cent illiterate and the per cent Negro for the Census Bureau's nine geographic divisions of the United States in 1930. They might want to see whether the geographic divisions which have a high percentage of Negroes also have a high percentage of illiteracy. These kinds of questions are concerned with ecological variables (percentages for geographical districts). The study of the regression between ecological variables (the per cent illiterate and the per cent Negro) is suitable for studying these kinds of questions.

* Based in part on research supported by the Office of Naval Research at the Statistical Research Center, University of Chicago.

¹ W. S. Robinson, "Ecological Correlations and Behavior of Individuals," *American Sociological Review*, 15, (June, 1950), pp. 351-357.

Another reason why sociologists study the regression between ecological variables is that they think that such a study may be used to make inferences concerning the behavior of individuals. In one sense these sociologists are incorrect and in another sense they are quite correct. The sociologist *cannot* conclude that the percentage of illiteracy among the Negro population is higher than the percentage of illiteracy among the non-Negro population simply because the correlation between percentage illiteracy and percentage Negro is .946 when the ecological areas are divisions. The percentage of illiteracy among the Negro population might be equal to the percentage of illiteracy among the non-Negro population and we still could obtain an ecological correlation of .946. In fact, the percentage of illiteracy among the Negro population might even be much less than the percentage of illiteracy among the non-Negro population and we still could obtain an ecological correlation of .946. A very high ecological correlation may be obtained when either (a) the individual correlation is close to 0, (b) the individual correlation is close to +1, or (c) the individual correlation is close to -1. Also, a very low ecological correlation may be obtained when either (a), (b), or (c) occur (i.e., when the individual correlation has almost any value at all). Hence, knowledge of the ecological correlations cannot *in general* be used to make inferences concerning the behavior of individuals. Since, however, sociologists continue to use ecological correlations to make such inferences, it is of interest to see in what circumstances these inferences are justified. We shall state the problem somewhat more abstractly than the color-illiteracy study so that the reader will easily see both the generality and the limitations.

Suppose a population can be divided into two mutually exclusive groups, N and W, with respect to one categorization and into two other mutually exclusive groups, I and L, with respect to another categorization. There is an unknown population parameter p , which we shall call the "average probability that a person who is in group N will also be in I." Similarly there is another unknown population parameter r which we call the "average probability that a person who is in group W will also be in I." We shall be in-

interested in ways to estimate these unknown population parameters p and r . If a sample of n people is obtained from N we would expect that np people in this sample would be in I ; i.e., $E\{u\} = np$, where $E\{u\}$ is the expected number of people in I appearing in the sample from N . If a sample of w people is obtained from W , we would expect that wr people in this sample would be in I ; $E\{v\} = wr$, where $E\{v\}$ is the expected number of people in I appearing in the sample from W . If a sample consisting of n people from N and w people from W is obtained, then we would expect that $np + wr$ people in the total sample would be in I ; i.e., $E\{u + v\} = np + wr$, where $E\{u + v\}$ is the expected number of people appearing in I in the total sample. Dividing both sides of this last equation by the total sample size of $n + w$, we find that the expected proportion of people appearing in I in the total sample is $E\{Y\} = Xp + (1 - X)r$, where X is the proportion $n/(n + w)$ of people from N in the total sample. We may rewrite this last equation in the form $E\{Y\} = r + (p - r)X$ or as $E\{Y\} = a + bX$, where $r = a$ and $p - r = b$. Since p and r are unknown population parameters, a and b will also be unknown population parameters. If several samples are obtained which have different values of X (different proportions of people from N in each sample), we might also observe the Y values in each sample (the proportions of people appearing in I in each sample). Plotting the values of Y and X for each sample on a graph gives us the usual scatter diagram (e.g., Figure 1 in Robinson's paper. We then could find the "best fitting" straight line for the scatter diagram by the method of least squares.² In so doing we will also find the least squares estimate for a (the Y intercept) and the least squares estimate for b (the slope). Since $a = r$, we might estimate r by the least squares estimate for a . Also since $a + b = p$, we might estimate p by the sum of the least squares estimates for a and b . These estimates of p and r will be unbiased.

We have seen how to estimate the unknown population parameters p and r using only ecological variables X and Y (the per cent appearing in N in each sample and the per cent in I in each sample). Hence we have used only ecological variables to determine the average probability p that a person who is in group N will also be in I , and the average probability r that a person who is in group W will also be in

I . These average probabilities are relevant to the "individual correlations" discussed by Robinson. Clearly the analysis of fourfold tables in his Table 2 would lead to better estimates of p and r than the estimates we have just obtained by means of a regression analysis of the ecological variables. However, in some cases, where it is too difficult to obtain the data in the cross-classified form of the fourfold tables, the regression method of estimation may be the best approach.

Let us review the assumptions which are necessary in order to carry out the preceding analysis: (a) we wish to estimate two population parameters p and r which do not differ from sample to sample; (b) we have the relation $E\{Y\} = Xp + (1 - X)r$, where X is the proportion of people from N in a sample and $E\{Y\}$ is the expected proportion of people appearing in I in a sample. If (a) and (b) are true, then the standard least squares approach will lead to unbiased estimates of p and r . If, furthermore, we are able to assume that (c) the values of Y are approximately normally distributed with the same variance for each value of X , then all of standard regression methods (test of hypotheses, confidence intervals) may also be applied. The reader is cautioned to examine assumptions (a), (b), and (c) in each particular case before proceeding with a regression analysis of the ecological variables.

We might wish to modify slightly the standard regression methods in order to deal with the special type of problem we have discussed. In our analysis we know that p and r must lie between 0 and 1 since they are "average probabilities." It is possible that both estimates of p and r obtained by the regression approach may not be between 0 and 1. For example, we may find from the ecological data that the regression approach leads to the estimates .2 and -.05 for p and r respectively. If this result was obtained we would be suspicious of our assumptions (a), (b), and (c), although it is possible to obtain such estimates even if these assumptions are satisfied. If these assumptions were satisfied, then we might estimate the value of r as 0 rather than -.05. We would then determine a new least squares estimate for p using the equation $E\{Y\} = Xp$.

In summary, we would like to emphasize that in general ecological regression methods cannot be used to make inferences about individual behavior. However, under very special circumstances the analysis of the regression between ecological variables may be used to make such inferences.

² W. J. Dixon and F. J. Massey, *Introduction to Statistical Analysis*, New York: 1951, McGraw-Hill Book Company, Inc.

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AN ALTERNATIVE TO ECOLOGICAL CORRELATION

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It was shown by Robinson that ecological correlation may be seriously misleading if it is used to infer corresponding individual correlation.¹ Herbert Menzel's "Comment" on Robinson's paper made the valid observation that "ecological correlations may be of great value even without reflecting individual correlations."² We make the additional point that, even if the investigator is concerned with the individual correlation, data classified by area may be of service, though ecological correlation is probably not the best way to handle them.³

Under favorable conditions, the individual correlation may be determined to a fair approximation, using principles set forth by Yule for testing consistence of data and for drawing inferences from incomplete data.⁴ This proposition is illustrated below with census tract data for Chicago taken from the publications of the 16th and 17th decennial censuses.

Table 1 shows the cross tabulation of occupation by color for the employed females of the city in 1940. Six per cent of the whites and 38 per cent of the nonwhites are employed in domestic service; or expressing these same facts in another form, the fourfold-point correlation between occupation and color is .289. In the situation where ecological correlation has been used in the past, and for which we are proposing an alternative technique, only the marginal totals of Table 1 are available, but there is also available a similar set of marginal totals for each of the areal subdivisions—say tracts—of the universe under study. Given just the marginals of Table 1, we may infer only that the percentage of nonwhites engaged in domestic service is between zero and 100, and that the fourfold r may be as low as $-.008$ and as high as $.898$. In tract 625 there are 877 employed females of whom 95 are white, 782 nonwhite; 239 are in domestic service and 638 in other occupations. At a maximum, therefore, only 239

nonwhites are in domestic service, while at least 144 (239-95) nonwhites must be in this occupation. Making a similar computation for each of the 935 tracts of the city and summing the maximum and minimum frequencies, we find that between 25 and 40 per cent of the nonwhites must be engaged in domestic service, and accordingly the fourfold r is between $.126$ and $.355$. (Note that it is necessary to fix maximum and minimum frequencies for only one cell of Table 1, since this cell, along with the marginals, determines the remaining frequencies.)

TABLE 1. EMPLOYED FEMALES BY OCCUPATION AND COLOR FOR CHICAGO, 1940

Color	Occupation		
	Other than Domestic Service	Domestic Service	All Occupations
White	359,011	23,241	382,252
Nonwhite	17,168	10,433	27,601
Total	376,179	33,674	409,853

An even closer approximation to the individual correlation is obtained using tract data for color of occupants and tenure of occupied dwelling units. We have the following marginals for the 949,744 occupied d.u.'s in the city in 1940: owner occupied, 230,975; tenant occupied, 718,769; occupied by whites, 873,479; occupied by nonwhites, 76,265. The city marginals are consistent with any fourfold r between $-.521$ and $.168$, whereas this range is narrowed to $.058$ to $.138$ on the basis of the tract marginals, as compared to the actual r of $.116$.

A third example is more realistic than the foregoing, in the sense that the city cross tabulation is not available. In 1950 the 1,106,119 d.u.'s of the city were distributed by year built and persons per room as follows: built later than 1929, 89,170; built in 1929 or earlier (including not reported), 1,016,949; less than 1.01 persons per room (including unoccupied and not reported), 943,112; 1.01 or more persons per room, 163,007. The city marginals permit variation between zero and 100 in the percentage of newer d.u.'s which are overcrowded, while this range is narrowed to 1.4-44.2 per cent with the tract data. Similarly the fourfold r is determined to be between $-.246$ and $.111$ from the tract data, although the wider limits of $-.712$ and $.123$ are consistent with the city marginals.

Table 2 summarizes the three illustrations. It will be noted that substantially closer ap-

¹ W. S. Robinson, "Ecological Correlations and the Behavior of Individuals," *American Sociological Review*, XV (June, 1950), pp. 351-357.

² *Ibid.*, p. 674.

³ We believe there are superior alternatives to correlation for strictly ecological studies as well; but that is the subject for another discussion.

⁴ G. Udny Yule and M. G. Kendall, *An Introduction to the Theory of Statistics*, 13th ed., London: Griffin and Co., 1947, Ch. 2.

TABLE 2. ILLUSTRATIONS OF INDIVIDUAL CORRELATIONS APPROXIMATED WITH AREA DATA

Variables Correlated and Parameters Computed	Basis of Calculation			
	City Cross Tabulation	City Marginals	City and Community Area Marginals	City and Tract Marginals
1. Color and occupation of employed females, 1940				
Percentage of non- whites in domestic service	37.8	0.0—100.0	21.1—44.5	25.1—40.7
Fourfold r	.289	-.003—.898	.126—.355	.165—.317
2. Tenure and color of occupants of occupied dwelling units, 1940				
Percentage of non- white occupied d.u.'s tenant occupied	92.5	0.0—100.0	75.4—98.0	84.1—95.8
Fourfold r	.116	-.521—.168	-.002—.153	.058—.138
3. Year built and persons per room of dwelling units, 1950				
Percentage of d.u.'s built since 1929 hav- ing 1.01+ per per- sons per room	0.0—100.0	1.4—44.2
Fourfold r	-.712—.123	-.246—.111

proximations to the individual correlation can be made from the marginal frequencies for the 935 census tracts than from the marginals for the 75 community areas, which are combinations of tracts. Although different systems of areal subdivision give different results, as is the case with ecological correlation,⁵ the criterion for the choice among these results is clear. The individual correlation is approximated most closely by the least maximum and the greatest minimum among the results for several systems of areal subdivision.

It should be noted that the range of possibilities determined from the tract marginals is not in the nature of a set of confidence limits for the true frequency or true correlation, but rather provides absolute upper and lower bounds for their values. Further, such a range need not be symmetrical about the actual value.

It is not suggested that the illustrations cited in this paper are typical. Furthermore, the investigator must decide in each case whether the approximation to the individual correlation is close enough for his purposes. However, the suggested procedure should probably be tried before adopting the unsatisfactory expedient of accepting the ecological correlation as an estimate of the individual correlation.

⁵ Robinson, *op. cit.*, p. 356

OPPORTUNITIES FOR SOCIOLOGICAL RESEARCH IN ALASKA

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Sociologists do not seem to know that they are missing a chance at very worthwhile research, and pleasant travel to get to it. To draw attention to the Territory's need for social research, the Alaska Division of the American Association for the Advancement of Science took special measures at the Third Alaskan Science Conference, held in September 1952, at which meeting the current social, political, and economic status of Alaska (chiefly the last two) and the status of social research were discussed.¹ This was planned without a single sociologist, because there were none in Alaska. At the University of Alaska no courses in sociology were being given except two offered apologetically by an anthropologist. President Terris Moore wanted to give more, but his school was small and he had many simultaneous

¹ The three papers given at a first-morning general session of the Third Alaskan Science Conference have been published in *The Scientific Monthly*, July 1953.

demands to expand or add departments. He has resigned as of June 1953, and his successor's policies are not known.

There has been a little research on the borders of sociology, chiefly by people in Federal or Territorial agencies. Examples are:

1. Social and personality study of TB patients—among whom there are always many Eskimos and Indians—as part of an interdisciplinary program of the Alaska Department of Health on its biggest problem, tuberculosis. The pertinent worker in this new undertaking was a clinical psychologist with some training also in sociology.

2. A study of recreational needs in Alaska, by the National Recreation Association, sponsored by the National Park Service.

3. A survey of mental health conditions and needs in the Territory, made by physicians, sponsored by the Department of Interior four years ago.

4. The Housing and Home Finance Agency said last year that it was making a special study of Juneau, Anchorage, and Fairbanks "regarding living conditions, distribution of income and housing needs."

5. The Bureau of Labor Statistics within the past ten years has made family income-and-expenditure surveys in the same three towns, and the Bureau of Indian Affairs made a comparable study in seven Indian villages in 1948. (Alaska is not included in the Census Bureau's Survey of the Labor Force or in the BLS consumer price index.) Among the economists of the team for the latter study, there was a social anthropologist with previous experience in Alaska.

A second approach to sociology has been made along the route of human geography, involving such studies as settlement patterns (town location), an ecological study of an Indian-occupied area on the Yukon River, and the Matanuska Valley project.

On another track social anthropologists in the past twelve years have studied traditional social organization and even some aspects of modern community structure in Point Barrow and on Nunivak Island (Eskimo), at Nome (White and Eskimo), in Tlingit villages (Indian), and in the Aleutian Islands (the Aleut branch of Eskimo). Recognizing that the boundary between sociology and social anthropology is hard to define, we still can say that only two anthropologists have come close to making a sociological study: on Eskimo acculturation in Nome, with special attention to the place of Eskimo women in its economy and society, and an analysis of the history, organization, and functions of the Alaska Native Brotherhood, a Territory-wide organization.

Except for one small study in the summer of 1952, no basic research in sociology has been done in this rapidly developing region. The only

specifically planned study of social interaction of which the writer knows was done by John James of the University of Oregon, who accompanied an anthropological expedition to the Aleutians. Professor James studied small-group behavior there.

It can be seen that most of the research has been of the old-style social-survey type or in the field of human ecology, in the fairly broad definition of the latter used by anthropologists.

Some reasons for immediate sociological attention to Alaska are:

1. The region offers an example of a phenomenon that is characteristic of this period—the new community grown from virtually nothing—but with qualities rare in today's brand-new towns: spontaneous, unplanned American settlement. These are not military or company towns, not dependent suburban towns laid out by real estate companies—although Alaska has a few, complete with shopping centers—but a 20th century repetition of frontier settlement. There were not many sociologists during the settlement of the West. There are sociologists today, hence not much excuse for their missing the current frontier.

Alaska's 77.4 per cent increase in total population from 1940 to 1950 was provided almost entirely by immigration. The number of aboriginal inhabitants increased only 4.3 per cent and, while the percentage increase of Alaskan-born whites was great, the number was still small. Yet total population increased from 72,500 to 128,600, 1940–50, and three years later is estimated by the Bureau of the Census to be about 180,000. There seems no doubt of at least a hundred and fifty per cent growth in the past thirteen years. Of that 128,600 enumerated in 1950, 26,000 had come from the continental United States in the preceding year alone.

The immigrants have come from all parts of the U. S., many from the South-Central states, supposedly bringing somewhat different subcultures and meeting the subcultures of Alaska. We have a problem, therefore, to ascertain which ones prevail in setting the social stratification, the ethnic relations, and other aspects of the new society. Or is the modern Alaskan culture already strong enough to prevail over all? No attempt is made here to outline any one piece of research. Each reader can see the specific requirements in his own field.

2. While tremendous defense construction (1,486,000,000 dollars, 1940–1952 inclusive), road construction and other building have brought people to Alaska, most of the newcomers are permanent settlers, not merely summer construction workers. Two factors, decline of the salmon industry and government maintenance of a fixed price for gold, which has

discouraged gold mining, are making Alaskans turn to new industries. If these, for example plywood and pulp manufacture and coal mining, do not develop so rapidly as expected and if Federal cuts in defense are stringent, there can be some deflation in Alaska's boom. Most of the growth seems permanent, however. The scramble for permanent facilities—schools, water systems, highway patrol, and the like—and the peculiar difficulties of organizing and financing these suggest the second reason for studying Alaska.

Alaska is still a Federal responsibility. It has no counties or townships. Instead of county commissioners and sheriffs, it has U. S. Commissioners and Marshalls. Unlike the emigrated Americans' position in Puerto Rico or Hawaii, or in an occupied area such as Western Germany, in Alaska they are the majority and feel that the country is theirs, yet feel institutional limitations. We see again the western frontier territory that expects to be a state, meanwhile improvising a substitute. It is a political body with colonial weaknesses but without the self-image of a colony. The formal social structures, especially when new, deserve attention.

3. Probably the best opportunity for social psychology is in the study of the American emigrant himself. There are also emigrants from Canada to Alaska. Most of the old sourdoughs were Scandinavians and Britishers (particularly Scots?) who contributed to the new culture not only such outward elements as the sport of curling but such basic characteristics as high education, still maintained in the new population. The median number of school years completed for the total white population of the Territory, according to the 1950 census was 12.2. This is about all that is known of the background of the white population. The emigrants are a self-selected group. What other factors besides education operate in the selection? Presumably these emigrants are socially different from those who settle in Paris or Rome. But perhaps the factors of escape from pressures at home are the same. The psychologist has a research job here. Occupation in the States, continuation or change of occupation in Alaska, rural or urban origin, expectations regarding Alaska, satisfaction or disillusionment, and a wealth of opinion questions will help give comparison and perspective on American society.

4. As with the original continental settlement and many present overseas settlements, there surrounds or mingles with the newcomers a variety of ethnic groups. There still are Eskimo and Indian villages with only three or four white residents in each. The greatest opportunity for the sociologist, though, especially

the educational sociologist, is in the town school systems under the Alaska Department of Education, not the Alaska Native Service, which are attended by all the races. These include in Anchorage and Fairbanks growing Negro communities. Comparison of matched Alaskan-born white children and immigrant white children and of the former with local Indian children in regard to many characteristics, from basic personality to speed in reading, would give important information on the rate of change, if any, in transplanted children, or on culture patterns, depending on how the research is designed.

Sociologist and anthropologist can work together in studying the Eskimos and Indians now living in the towns. Unlike Indians in the States, they have not gone through a Reservation period. They are going from independent self-directed work to industrial and construction work, for example on the Alaska Railroad. Much-needed study of crime in the various ethnic groups and of several other social problems can give practical help to the territorial institutions that, in structure and manpower, are inadequate to the new growth.

5. Ecology: although the present tendency in Alaskan rural areas is for settlement in neighborhoods rather than in isolated homesteads, the latter still occur. Besides the selective factors in the natural environment, what are the selective factors in the social environment? One could narrow the research problem by studying only the servicemen stationed at a large base like Ladd Field who build cabins in off-duty hours and who homestead, the ones who stay in Alaska compared with those who go home. The possibilities of matching the homesteaders with others in their outfits are obvious.

Some of the towns already need slum clearance and urban redevelopment and the vigorous Alaska Housing Authority is trying to meet the need. This is mentioned to indicate the "field," the community: almost anything from shacks-towns and tall apartment buildings to the home in the wilderness.

6. Finally, the sociologist, psychologist and social anthropologist have a cooperative task in describing this culture that is new, not merely transplanted. Here is one small example: The Arctic Health Research Center in a study of food supplies and habits in Anchorage found that its residents eat very well. Their consumption of meat is especially high. This seems to be due partly to cold winters, to availability of game and fish in addition to imported frozen meat, and to the favorable price of meat. (All food prices are high, but compared with prices in the States, meat is not as expensive as are many other foods in Alaska.) There are appar-

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ently still other factors not so easy to see: custom, going back to the largely meat diet of the native peoples and the first settlers, and the "hearty meal" as a symbol of rugged well-being, even if one spends all day in a well-heated office and eats lunch in a modernistically decorated coffee-shop. Alaska is unexpectedly urban. It has added the airplane to the dog-team without going through a Model-T stage. Are the people's attitudes and habits a corresponding mixture? No one knows.

There are difficulties in research in Alaska. The principal one is cost. To the estimate of a project in the States, one must add 25 per cent for living expenses and most other expenses for the same study in Alaska. One large item in most sociological research today—statistical treatment of the data—will cost little more if the material is sent or brought to the States for this. There are offsetting advantages in that one is traveling in American territory, need not have passport and visa, and seldom needs an interpreter. Whether it will cost more to go to the new industrial community at Ketchikan, where a large pulp-making plant is being built, than to go to a new industrial site in the Carolinas, depends on where one starts. Alaska is in the natural sphere of inquiry of the northwestern states.

For a summer's fieldwork in any of the towns, one would have no extra expenditure for special clothing or other equipment. To live comfortably in the remoter villages, one should have special gear. Sociologists are needed in the most urban places, though, and can leave the Eskimo villages to the anthropologists.

Finally, the work must be self-initiated.² Probably no one will ask for him, but once there, the sociologist will meet certain elements of the Alaska code that already are known; friendliness, hospitality, readiness to learn and to improve, if the visitor is not condescending.

The post war trend among the young sociologists who are taking advantage of Fulbright funds or who are going to places like Puerto Rico should be extended. Even before World War II, rural sociologists made a good start in getting outside their own culture. Loomis, T. Lynn Smith, Whetten, Carl Taylor went into Latin America. Now, by means of Point Four,

sociologists are going to India, Indonesia, and other distant areas. Compared with those regions, Alaska is on the front doorstep.

A CAMPARISON OF ATTITUDES OF ENLISTED AND COMMISSIONED AIR FORCE PERSONNEL *

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and

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In a study of attitudes toward punishment of airmen found guilty of desertion, a sample of 2300 commissioned officers at 17 air bases in three major air force commands showed a significantly greater tendency to recommend clemency and psychiatric treatment than did a sample of 1000 airmen of the first three grades at five bases of the Strategic Air Command. The difference between commissioned and enlisted personnel persisted when officers and airmen in the same command were compared.

Each respondent was asked a series of 18 questions about the proper punishment of an airman found guilty of desertion. All of the questions appeared on a single page and were preceded by the following hypothetical situation:

"The Commanding General has appointed you to a court martial board to hear cases of violation of the Uniform Code of Military Justice.

"One of the defendants brought before the court was James Allen, charged with desertion. He had been arrested three months following his desertion, was brought to trial, and pleaded guilty. Proof of his violation was clear-cut, and the court returned a verdict of guilty as charged.

"Allen had pleaded in extenuation that he deserted because of an overpowering fear of combat flying. He presented to the court an affidavit from a psychiatrist Medical Officer who had examined him a few days before he deserted. It was the psychiatrist's judgment that Allen was suffering from a severe neurosis and emotionally sick, though not insane in the legal sense."

Respondents were asked to state the punish-

² For information on public or private agencies through which one could work and whose cooperation one probably would want, write to the Alaska Division, Office of Territories, U. S. Department of the Interior, Washington; Office of the Governor (Hon. Frank Heintzleman), Juneau; the Alaska Delegate to Congress, the Honorable E. L. Bartlett, New House Office Building, Washington; or to the President, University of Alaska, College, Alaska.

* This research, carried out at the Harvard Laboratory of Social Relations, was supported in part by the United States Air Force under Contract AF33 (038)-12782 monitored by the Human Resources Research Institute. Permission is granted for reproduction, translation, publication and disposal in whole and in part by or for the United States Government.

TABLE 1. PERCENTAGES OF AIRMEN AND OFFICERS RECOMMENDING CLEMENCY AND PSYCHIATRIC TREATMENT IN RESPONSES TO ELEVEN ITEMS*

Assume the defendant had been:	Per cent recommending clemency and psychiatric treatment	
	Airmen	Officers
1. An aerial gunner with maximum combat missions who had requested relief from flying duties	77%	90%
2. A corporal, with no previous convictions	80%	80%
3. Staff Sergeant, with record of having completed heavy bomber tour of duty during the war	69%	80%
4. Master Sergeant with 13 years' service	49%	61%
5. Staff Sergeant, with five years' non-flying service and no previous convictions	41%	60%
6. Sergeant, awaiting shipment to a combat base	33%	56%
7. † Stationed in the U. S. with no combat record	24%	42%
8. † Private, recently reduced from the grade of Sergeant for striking a superior officer	17%	35%
9. Airman, with a long record of petty offenses in the squadron	12%	26%
10. Private with three previous convictions for AWOL	11%	24%
11. Private who had told some other airman he was trying to get a "psycho" discharge	12%	19%

* Slight differences in the items were introduced in the Officer and Airmen's questionnaires to permit consistency of the questionnaire.

† Item not used in the computation of the scale.

ment they would approve for the defendant under eighteen different conditions. Four response categories were presented and the respondents were instructed to check only one category for each of the eighteen conditions. They were permitted to choose between: recommendation of immediate shipment to combat; confinement; fine; recommendation of clemency and psychiatric treatment. Eleven of the questions were the same for the officers and the enlisted men, and the responses to these provide the primary data for this report. The responses to the eleven questions are presented in Table 1.

In items where the defendant was described as having a good prior record, as many as eighty per cent of the officers and airmen said they would recommend clemency and psychi-

atric treatment for an airman found guilty of desertion, rather than one of the more severe punishments of fine, confinement, or a recommendation of shipment to combat. When the defendant was described as having told other airmen he was trying to get a "psycho" discharge, a fifth of the commissioned officers and a tenth of the airmen still would choose to recommend clemency and psychiatric treatment rather than a more severe form of punishment. These data indicate an apparent widespread attitude among both officers and airmen toward leniency in cases of desertion involving extenuating circumstances of an emotional or psychiatric nature. The tolerance of fear of combat as a legitimate basis for clemency in cases of desertion is pronounced among a large proportion of Air Force personnel, as evidenced by verbal responses to questionnaire items.

An H-type Guttman scale was constructed using nine of the eleven items.¹ The scale, with a coefficient of reproducibility of 99.5 stands up on both the sample of officers and the sample of airmen with equally high reproducibilities, thus establishing the internal validity of the scale. The percentage distribution of airmen and officers by scale type is shown in Table 2.

The mean scale score of the officers was 2.67

TABLE 2. DISTRIBUTION OF AIRMEN AND OFFICERS BY SCALE TYPE

Scale type	Per cent in each scale type		Total
	Airmen (N=1006)	Officers (N=2330)	
4. (Most in favor of clemency and psychiatric treatment)	9.2	20.7	17.2
3	29.1	38.1	35.4
2	43.0	28.7	33.0
1 (Least in favor of clemency and psychiatric treatment)	18.7	12.5	14.4
Total	100.0	100.0	100.0

¹ For a full statement of the H-technique of scale construction, see Stouffer, S. A., Borgatta, E. F., Hays, D. G., and Henry, A. F., "Technique for Improving Cumulative Scales," *Public Opinion Quarterly*, Summer, 1952.

and of the airmen was 2.29. The difference between the mean scale scores of officers and airmen was significant beyond the .001 level.

The greater leniency of officers is not a function of command differences. No significant difference occurred among commissioned officers of the three commands represented in the sample.

Data are insufficient to test adequately alternative explanations of this result. The status

position of the commissioned officer is higher than that of the non-commissioned officer. Members of the higher status group express greater tolerance of weakness arising from psychiatric difficulties than members of the lower status group. The result confirms a widespread observation within the Air Force that enlisted members of military courts consistently vote for harsher sentence than commissioned members.

COMMUNICATIONS AND OPINION



REPLY TO KINGSLEY DAVIS

We advance our science by developing and expanding the existing theoretical statements formulated by our fellow scientists. It was with this aim in mind that I took the article by Davis and Moore as the best statement of a given theoretical position. I, therefore, regret that Mr. Davis has viewed my efforts in a somewhat different light. That I chose the Davis-Moore article should be ample evidence of my respect for it.

In view of Davis' objections to my suggested revisions, it now becomes possible to point up the central issues which ought to be joined.

1. Does the fact that a given institutional pattern is universal necessarily imply that it is positively functional? Are there not numerous universals which represent structurally built-in limits on human efficiency? It is important that this alternative characterization of universal patterns be offered, since Mr. Davis insists that only those patterns survive which prove to be "best for society." Clearly, he cannot use this argument in accounting for existing stratification systems without applying it equally to such other matters as institutionalized human ignorance, war, poverty and magical treatment of disease.

2. Does the universality of an institutional pattern necessarily testify to its indispensability? In the case of stratification, does Mr. Davis really mean to imply that the functions of locating and allocating talent cannot be performed by any other social arrangement? Since a theoretical model *can* be devised in which all other clearly indispensable major social functions are performed, but in which inequality as motive and reward is absent, how then account for stratification in terms of structural and functional necessities and inevitabilities?

3. An essential characteristic of all known kinship systems is that they function as transmitters of inequalities from generation to generation. Similarly, an essential characteristic of all known stratification systems is that they employ the kinship system as their agent of transmission of inequalities. In effect, of course, this is saying the same thing in two apparently different ways. The fact is that kinship and stratification overlap in all known societies. One cannot fully describe any given kinship system

without implicitly including the transmission of inequalities if one assigns the function of "status-placement" to kinship. Similarly, one cannot describe any stratification system in operation without implying the generational transmission of inequalities, if one includes the dynamics of inequality.

To the extent that this is true, then it is true by definition that the elimination from kinship systems of their function as transmitters of inequalities (and hence the alteration of the definition of kinship systems) would eliminate those inequalities which were generation-linked.

What puzzles me, therefore, is how one can assign the "villainy" of the act of transmission of inequalities to the kinship system and insist, in turn, that the stratification system is pristine in this regard.

4. Obviously, the denial to parents of their ability and right to transmit both advantages and disadvantages to their offspring would require a fundamental alteration in all existing concepts of kinship structure. At the least, there would have to be a vigilant separation maintained between the unit which reproduces and the unit which socializes, maintains and places. In theory, this separation is eminently possible. In practice, it would be revolutionary.

One of its likely consequences would be a thorough-going alteration in the motives which impel men to high effort. It was precisely to this point of alternative possible motives that I addressed much of my original argument. Dr. Davis argues against the motive of "esteem" as being inadequate on the grounds that esteem alone tends to produce a static society. This may be true by definition. Aside from that possible source of verity, there is no empirical evidence which will support Dr. Davis' contention.

Further regarding alternative motivations, Davis argues that (a) if everybody elected to do just what he wanted to do, the whole population would wind up in only a few types of position; and (b) any sociologist should know the inadequacy of unrewarded altruism as a means of eliciting socially adequate behavior. These statements are not true even by definition; and certainly we have no sound empirical studies which will support them as Davis puts them.

It does not seem to be the best thing for a

growing science to shut the door on inquiry into alternative possible social arrangements.

5. Finally, my central argument was to the effect that there are strategic functions of stratification systems which were overlooked in the Davis-Moore article. I tried to identify a number of the operations of stratification which seemed clearly to render inefficient the process of location and allocation of talent. Davis chooses in his rejoinder either to ignore these disfunctions or to attribute them to kinship rather than to stratification. But they are there clearly to be seen. Added in with the positive functions which have been identified, we get a *mixed* net result of inequality in operation.

Of course, all institutional arrangements of any complexity are bound to be mixed in their instrumentality. It is the recognition of this

mixture, and the emphasized sensitivity to the undesired aspects, which impels men to engage in purposeful social reform. In turn, social scientists have been traditionally concerned with the range of possible social arrangements and their consequences for human society. One is impelled to explore that range after probing deeply into whether a given arrangement is unavoidable and discovering that it is not. One is even more impelled to such exploration when it is discovered that the *avoidable* arrangement is probably less efficient than other possible means to the stated end. It was toward such further probing that I directed my original remarks. Joining the issues here raised will, perhaps, help probe more deeply.

MELVIN TUMIN

Princeton University

OFFICIAL REPORTS AND PROCEEDINGS



PROCEEDINGS OF THE 48TH ANNUAL MEETING OF THE AMERICAN SOCIOLOGICAL SOCIETY, HELD AT BERKELEY, CALIFORNIA, AUGUST 30, 31, SEPTEMBER 1, 1953

MINUTES OF COUNCIL MEETINGS

First Meeting of the 1953 Council— August 29, 1953

The first meeting of the 1953 Council was called to order on August 29, 1953, at 10:15 a.m. by President Stouffer, with the following members present: Herbert Blumer, Florian Znaniecki, Robert E. L. Faris, Leonard S. Cottrell, Jr., Dorothy S. Thomas, Read Bain, Robin M. Williams, Jr., Philip M. Hauser, Calvin F. Schmid, Leonard Broom, Thomas D. Eliot, August B. Hollingshead, Katharine Jocher, Harry E. Moore, Howard W. Beers, Margaret Jarman Hagood, Donald R. Young, Matilda White Riley, *ex-officio*.

The following committee reports were read and accepted: Membership Committee, Wellman J. Warner, Chairman; Committee for Liaison with the National Council for the Social Studies, Leslie D. Zeleny, Chairman, the Committee to be continued for another year; Committee on Standards and Ethics in Research Practice, Alfred McClung Lee, Chairman, with a new Committee to be appointed for the coming year as recommended by the outgoing Committee; Committee on Relations with Sociologists in Other Countries, Robert C. Angell, Chairman; Committee on Research, Fred L. Strodtbeck, Chairman; Committee on Training and Professional Standards, Calvin F. Schmid, Chairman; Program Committee, Leonard Broom, Chairman.

In addition, the following reports from representatives were received: Raymond Bowers to the American Association for the Advancement of Science (Dr. Bowers' report as Chairman of a special committee on the relationship between sociology and the American Association for the Advancement of Science was also received); Elbridge Sibley to the American Documentation Institute with the further action, upon recommendation, that the Society's representation be suspended; Hortense Powdermaker to the American Council of Learned Societies, with a vote that the Society's affiliation be continued;

Lowry Nelson to the Social Science Research Council.

The Council continued its work through lunch, whereupon the reports from the Editor and the Secretary were heard and accepted. It was moved and voted to appoint a committee to make policy proposals to the Council regarding the receipt of gifts and the issuance of awards by the Society. This committee was also instructed to consider means of stimulating interest in awards which may be open to competition. In regard to the MacIver Award, it was voted to defer any award for the coming year and to instruct the new committee to reconsider the details of the award and to report its recommendations back to the Council.

The report of the Executive Officer was accepted with commendation.

It was voted that the petition for affiliation with the Society from the Society for the Study of Social Problems should be submitted to the membership for action by mail ballot. It was also voted that the ballot mailing to the membership should include, in addition to the petition, a statement from a special committee of the Council of any problems and general policy issues which may be involved in such an affiliation. In this way it was felt that the membership would be in a position to cast an informed vote.

The Council reaffirmed the policy that all committee reports should be reported in the pages of the *American Sociological Review*, although the Secretary was empowered to condense such reports wherever necessary in the interest of saving space. The Secretary was also authorized to make similar condensations of reports at the Business meetings of the Society.

It was voted to add *Social Problems* to the list of periodicals which are offered to the membership at special subscription rates. In the case of *Sociological Abstracts*, however, action was deferred until a regular publication schedule was guaranteed. Meantime, it was voted to give the editors of *Sociological Abstracts* encouragement and publicity.

It was voted to refer the problems of copy-

right and permissions for reprinting material from the *Review* to the Publications Committee for special study and for early report to the Executive Committee.

It was voted to offer copies of the recording of the remarks of past Presidents of the Society to the membership at \$2.50 per record.

A Resolutions Committee for the 1953 meetings consisting of Ellsworth Faris, Chairman, Stuart Queen, and Edgar Thompson was approved.

The meeting was adjourned at 5 p.m.

*Second Meeting of the 1953 Council—
August 30, 1953*

The second meeting of the 1953 Council was called to order by President Stouffer on August 30, 1953, at 5 p.m. with the following members in attendance: Florian Znaniecki, Herbert Blumer, Robert E. L. Faris, Dorothy S. Thomas, Read Bain, Robin M. Williams, Jr., Philip M. Hauser, Calvin Schmid, Thomas D. Eliot, Charles Hutchinson, Katharine Jocher, Howard W. Beers, Alfred McClung Lee, Donald R. Young, Harvey J. Locke, Matilda White Riley, *ex officio*.

The minutes of the first meeting of the Council were approved as read.

It was voted to increase the part-time salary of the Executive Officer from \$3,000 to \$3,500, with commendation.

In connection with the 1955 meetings, it was voted to leave the final decision to the Executive Committee with the following guidance from the Council: 1) that the meetings should be held east of the Alleghenies, 2) that a university setting should be sought, preferably one where hotel facilities are also available, and 3) that the same early September date should be arranged.

It was voted to contribute not less than \$100 to the running expenses of the International Sociological Association, in addition to continued cooperation between the Society and the Association with respect to such matters as directories, meeting plans, and international bibliographies. In this connection also, Stuart Dodd, who had arrived only the day before from the meetings of the International Sociological Association in Liege, brought greetings and messages from the Association. Plans were tentatively discussed for integrating the dates of the 1956 meetings of the two organizations. It was announced at this point that cabled greetings had been exchanged between the two meetings.

It was voted to authorize the appropriate officer of the Society to conclude an agreement with the Russell Sage Foundation which would make feasible the preparation and publication

of a series of bulletins devoted to critical review of recent publications in such applied fields as penology, social psychiatry, health services, counselling, community organization, etc.

Two committees were elected as follows:

The Committee on Affiliation: Leonard S. Cottrell, Chairman, Thomas D. Eliot, Robert E. L. Faris, Leonard Broom, Howard W. Beers.

The special Committee on Awards: Katharine Jocher, Chairman, Wellman J. Warner, August B. Hollingshead, Paul Wallin, Herbert Blumer.

The report of the Committee on the Britt Award was received and accepted with the instruction that this report be given at the first Business Meeting.

A resolution from Thomas Monahan calling for the creation of a special committee on marriage and divorce statistics was referred to the Society's Committee on Social Statistics. An expression of the Society's great interest in this problem will, however, be communicated to Dr. Monahan.

The meeting was declared adjourned at 6 p.m.

*The First Meeting of the 1954 Council,
September 1, 1953*

The first meeting of the 1954 Council was called to order by President Znaniecki on September 1, 1953, at 3 p.m. with the following members present: Donald R. Young, Robert E. L. Faris, Dorothy S. Thomas, Samuel A. Stouffer, Philip M. Hauser, Calvin F. Schmid, Margaret Jarman Hagood, Harvey J. Locke, Leonard Broom, Thomas D. Eliot, Charles Hutchinson, Katharine Jocher, Alfred McClung Lee, Harry E. Moore, Matilda White Riley, *ex officio*.

President Znaniecki outlined the plans for the 1954 meetings. Following this a Committee on Committees, appointed by Professor Znaniecki and consisting of Howard Beers, Philip Hauser, Florian Znaniecki, and John Riley, *ex officio*, brought in suggested rosters for the various committees, a slate of 4 candidates for the two vacancies on the Executive Committee, a slate of 3 candidates and 3 alternates for positions as Assistant Editors of the *Review*, and one candidate for the post of representative to the American Council of Learned Societies. The Committee on Committees made note of its position as an *ad hoc* committee of the Council and pointed out that any and all of these rosters and slates were open to rejection, addition, and revision by the Council. It also took note of the fact that the slate of 3 candidates from which the Council was to elect a Director of the

Social Science Research Council was prepared by the Social Science Research Council in accordance with their by-laws. After considerable discussion and several changes in the various rosters and slates, the following actions were taken:

Robert E. L. Faris was elected as a Director of the Social Science Research Council for a three-year term.

Robert C. Angell was elected as the Society's representative to the American Council of Learned Societies for a three-year term.

Margaret Jarman Hagood and Philip M. Hauser were elected to the Executive Committee for two-year terms.

Guy E. Swanson, Harvey J. Locke, and Donald Cressey were elected as Assistant Editors of the *Review* for three-year terms.

In addition, the following committees were elected:

Budget: Philip M. Hauser, Chairman, Conrad Taeuber, Donald R. Young.

Classification: Stuart A. Queen, Chairman, (in addition to Talcott Parsons and Milton Barron who were previously elected).

1955 Program: (in addition to *ex officio* members) Edmund Volkart, Conrad Taeuber, and a third member to be elected by the Executive Committee.

Training and Professional Standards: Leonard S. Cottrell, Jr., Chairman, Calvin F. Schmid, Elbridge Sibley, Samuel A. Stouffer, Robin M. Williams, Jr., Albert J. Reiss, Jr., William H. Sewell, Leo Goodman, Harry Alpert, Leslie D. Zeleny, Ray E. Baber, Mabel Elliott.

Ethical Principles in Research: Oswald Hall, Chairman, Robin M. Williams, Jr. Rudolf Heberle, Richard O. Lang, Leslie Kish, Charles Y. Glock, Preston Valien, Glaister Elmer, Carroll D. Clark, S. M. Miller, Alfred R. Lindesmith.

Relations with Sociologists in Other Countries: E. Franklin Frazier, Chairman, Robert C. Angell, Nels Anderson, Conrad Arensberg, Norman S. Hayner, Irwin T. Sanders, W. Rex Crawford, Thomas D. Eliot, John P. Gillin, Howard Becker, Charles P. Loomis, Leo Lowenthal, Paul Massing, Henry J. Meyer, Talcott Parsons, Bryce Ryan, Jesse F. Steiner, Irene Taeuber, Kurt H. Wolff, Byron Fox, Thorsten Sellin, Dudley Kirk, Donald Taft, Reinhard Bendix, Carl C. Taylor, Paul Honigsheim.

Social Statistics: Henry S. Shryock, Jr., Chairman, Hugh Carter, Irene Taeuber, Seymour Wolfbein, Donald Bogue, Felix E. Moore, Jr., Stuart C. Dodd, Robert F. Winch.

Liaison with National Council for the Social Studies: Leslie D. Zeleny, Chairman, Wallis Beasley, Claude C. Bowman, Joseph Gittler, Leo A. Haak, C. G. Swanson, Stanley P. Wronski, W. Seward Salisbury. Other members are to be added later in association with members to be selected by the National Council for the Social Studies.

In addition, the following committees, appointed by the President, were approved by the Council:

Research: Fred L. Strodtbeck, Chairman, Paul C. Glick, Mozell Hill, Sigurd Johansen, Paul Meadows, H. Ashley Weeks, Arthur L. Wood.

Membership: Wellman J. Warner, Chairman. The large working Committee to be continued with special commendation for the Chairman.

Nominations and Elections: Robert C. Angell, Harvey J. Locke, Joyce O. Hertzler, Richard T. LaPiere, M. C. Elmer, George B. Vold, Wilbert E. Moore, Logan Wilson, Meyer F. Nimkoff, Lloyd A. Cook, J. Howell Atwood, Guy B. Johnson, Alfred McClung Lee, Kimball Young.

"Consultation": (to serve as a clearing house for 1954 Annual Meeting papers) August B. Hollingshead, Chairman, Arthur Wood, Milton M. Gordon, Karl F. Schuessler, Ivan C. Belknap, Theodore Caplow.

Donald R. Young, as President-Elect, was requested to make recommendations to the 1955 Council on the selection of the next Editor.

Finally, an addendum to the report of the Committee on Training and Professional Standards was accepted with the stipulation that it be referred to the new Committee.

The meeting was adjourned at 5:15 p.m.

Respectfully submitted,
JOHN W. RILEY, JR.
Secretary

MINUTES OF THE BUSINESS MEETINGS OF THE SOCIETY

The first Business Meeting of the Society was called to order at 11:00 a.m. on August 31, 1953, by President Stouffer.

The Minutes of the last meeting, as reported in the December 1952 issue of the *Review*, were accepted as published.

The reports of the Secretary and of the first two meetings of the Council were accepted save for one additional action taken by the Council which provides for an increase in the part-time salary of the Executive Officer.

The report of the Editor was accepted and a further announcement made of a special issue of the *Review* to be devoted to research on small groups. Manuscripts and suggestions are invited from the membership. These should be sent to the Editor before March 1st.

Professor Muzumdar then took the floor and read several suggestions concerning the content and management of the *Review*. These have been taken under advisement.

The report from the Executive Officer was read and accepted.

The report from the Program Committee,

Leonard Broom, Chairman, was heard and accepted with special commendation for the several successful innovations of this year's meetings.

The composition of the 1954 Committee on Nominations and Elections, as appointed by President-Elect Znaniecki, was announced.

The report of the Membership Committee was accepted as read by the Chairman, Wellman J. Warner.

It was announced that the registration had reached 490.

The meeting was declared adjourned at 12:15 p.m.

The second Business Meeting of the Society was called to order at 11 a.m. on September 1, 1953, by President Stouffer.

The Minutes of the last meeting were approved as read.

The reports of the following committees were read and accepted: (These are published in full below).

Research, Fred Strodtbeck; *Standards and Ethics in Research Practice*, Alfred McClung Lee; *Liaison with National Council for the Social Studies*, Leslie D. Zeleny; *Relations with Sociologists in Other Countries*, Robert C. Angell; *Training and Professional Standards*, Calvin F. Schmid; *The Britt Award*, Wellman J. Warner; *Relationship between the Social Sciences and the American Association for the Advancement of Science*, Raymond V. Bowers.

In addition, reports were heard from the Society's delegate or representative to the American Association for the Advancement of Science, the American Council of Learned Societies, the Social Science Research Council, the American Documentation Institute. (These are also published elsewhere in the Proceedings).

Professor Ellsworth Faris, for the Resolutions Committee, gave the following report which was unanimously accepted:

Your Committee has received no resolutions of a controversial nature and therefore has only the pleasant task of expressing in a formal manner our thanks to some of those who have put us in their debt by their gracious service.

We congratulate all those who are responsible for the decision to hold this meeting here by the Golden Gate. The large attendance is proof of the wisdom of their choice.

Our thanks are due to the University of California for making available the unexcelled facilities provided in the ample and spacious buildings.

To the seven members of the Committee on Local Arrangements we owe much thanks, and especially to their efficient and urbane chairman, Professor Judson T. Landis.

The Directory of Members is of the highest value to our membership, and the fact that it was ready in time for this meeting is due to the strenuous efforts of the secretary of the Executive Office, Miss Miriam Alpert, who deserves the thanks of us all.

The Executive Office puts us under obligation twelve months in every year, and so we have perennial thanks to give to that admirable pair who live the life of Riley.

We especially thank the management of International House for making available their facilities. We must record the expressed dissatisfaction of married couples who found the separation of man and wife an unexpected inconvenience. Recognizing the necessity of the arrangements on this occasion, it is hoped that future hosts will not be forced to do the same. But again we say: Thanks to the International House.

We thank the press of San Francisco for their interest in our meetings. If the reporters, in their stories, had difficulty in translating Sociologese into English, the fault is partly ours.

Respectfully submitted,
EDGAR THOMPSON
STUART A. QUEEN
ELLSWORTH FARIS, *Chairman*

In a discussion of the work of the Committee on Training and Professional Standards, Professor Valien suggested that the experience of the American Chemical Society be examined for relevant suggestions.

Finally, various suggestions and comments were made regarding the annual programs of the Society. Among other matters it was proposed that the time of each paper be scheduled to permit listeners to go from one session to another with some assurance of hearing desired papers. It was also suggested that more attention on the program might be paid to such topics as teaching and experimental sociology. It was finally suggested that one paper, previously circulated to the membership, might be presented for open discussion.

The meeting was declared adjourned at 12:15 p.m.

Respectfully submitted,
JOHN W. RILEY, Jr.
Secretary

REPORT OF THE SECRETARY

The affairs of the Society subsequent to its last Annual Meeting, insofar as they involved major policy and financial decisions, have been conducted by the Executive Committee and the Council, and in the case of the elections, by a mail ballot of the membership.

These various actions, for the record, are reported herewith:

Approval of Minutes of Council meeting at Atlantic City. In view of the fact that the time of the Council meeting preceded the final 1952 Business meeting, all actions taken by the Council were subject to ratification by mail ballot of all members. These were duly approved in September and became part of the record of the 1952 meetings.

The 1953 Budget. In December, on the recommendation of the Executive Committee and the Budget Committee, the Council voted approval of the Budget for 1953. This called for sale of the 1953 Directory and estimated a slight decrease in members and subscribers during the year.

Election of Donald Bogue as an Assistant Editor of the Review. In February, due to the untimely death of Paul Hatt, the Council, voting on a slate of five candidates prepared by the Executive Committee, elected Donald Bogue to serve for the balance of 1953 and for 1954.

Local Arrangements Committee for 1954 meetings. In May, the Council, on recommendation of the Executive Committee, voted approval of the following Committee:

DEAN ROBERT L. BROWNE, *Chairman*
ROBERT DUBIN
J. E. HULETT, JR.
ERNEST SHIDELER

The MacIver Award. In May the Council voted to accept the report of the Committee on the MacIver Award, but with suggestions that the following matters be discussed by the Council at its next meeting:

- 1) that the amount of the Award be fixed by the Council
- 2) that the Award be limited to young members of the Society
- 3) that the fund be augmented

Revision of the 1953 Budget. On the recommendation of the Budget Committee the Council in June voted to approve the mid-year revision of the Budget. A slight revision in the estimated costs of the 1953 Directory and a transfer of \$600 from the *Review* printing budget to the Editor's budget constituted the only major changes in the original budget. According to the report to the Budget Committee, it would appear that the Society's finances are well on the safe side this year.

The following actions by the Executive Committee should also be reported:

The Berkeley meetings. It was suggested to members that uncertainty as to attendance should not deter them from submitting ab-

stracts of papers. A registration fee of \$3 was approved to cover, among other things, a mimeographed copy of the abstracts of all papers, the buffet supper, and incidental expenses. A plan was also worked out for coordination with the Society for the Study of Social Problems.

The 1954 Meetings. The 1954 meetings are to be held at the University of Illinois, September 8 through 10, 1954, and a slate of candidates for the Local Arrangements Committee was proposed to the Council for approval.

A new publications series. The Executive Committee, on the recommendation of the Executive Office, voted to authorize the President to undertake preliminary negotiations with the Russell Sage Foundation for a joint project to publish a series of bulletins of professional interest. The President, in turn, instructed the Executive Office to pursue the matter, and it is hoped that a plan will be ready for action at the Berkeley meeting.

Miscellaneous matters pertaining to awards. In general, it was decided that arrangements to accept donations of funds for awards should be handled by the Executive Committee with special committees set up for the administration of such funds. This is in line with the procedure established for the MacIver Award. Thus, it was voted to accept a donation from the Britt Foundation to be used as a grant-in-aid for the best project in the field of social psychology submitted by a student member. A Committee, under the chairmanship of Wellman Warner, has been set up for this award.

Miscellaneous financial matters. Apart from its various recommendations to the Council in connection with the 1953 Budget, the Executive Committee approved an increase in the subscription rate of the *Review* from \$5 to \$6 starting with the 1953 volume, a charge of \$1 for mimeographed copies of the lists of current research projects, and a general approval for enclosing affiliated society materials in Society member mailings within the limits of the Society's budget.

Book review policy. At the request of the Editor, an advisory opinion as to the book review policy of the *Review* was formulated and published in the December 1952 issue. Further recommendations are to be considered by the Publications Committee.

Policy on referendum ballots. Issues to be voted on by the membership by mail ballot should be presented as follows: 1) an objective statement of the issue by the Secretary, 2) a statement by a committee of three on each side of the issue, each to have the same amount of space.

Miscellaneous committee actions. Jessie Ber-

nard was appointed chairman of the Publications Committee; the Chairman of the Committee on Social Statistics was authorized to appoint a subcommittee on marriage and divorce statistics; and Frank Lorimer was appointed as the Society's representative at the Public Health Conference on Records and Statistics.

Although the report of the Committee on Nominations and Elections is published in full in the August *Review*, the outcome of the balloting is set forth herewith for the Secretary's record:

President-Elect	DONALD R. YOUNG
First Vice-President	JESSIE BERNARD
Second Vice-President	IRA DE A. REID
Committee on Publications	OTIS DUDLEY DUNCAN
Council	GORDON W. BLACKWELL, MARGARET JARMAN HARGOOD, EVERETT C. HUGHES, HARVEY J. LOCKE

Further details of the Society's activities, as they relate to such organizations as the Social Science Research Council, the American Association for the Advancement of Science, as well as the activities carried out by the established committees of the Society, will be reported by the Society's representatives or committee chairmen and are not part of this record.

In respect to its affiliated societies, the Society has continued to cooperate in various ways to the end of heightening sociological interests and activities. This has been accomplished by publishing announcements of regional meetings in the *Review*, sending Society representatives to these meetings, and in general working toward closer relations with affiliated groups.

Details concerning the financial position and membership structure of the Society are part of the report of the Executive Officer. In line with past procedure, however, it is the solemn function of the Secretary to record the death of the following members:

J. Neely Boyer	Lea Mandelman
Gladys Bryson	Henry B. Martz
Lucille Eaves	Paul R. Stevick
Seba Elbridge	Herman Weinheimer
Paul K. Hatt	Francis S. Wilder
F. K. Kruger	Julian L. Woodward
Solomon Landman	Erle F. Young
J. P. Lichtenberger	

Finally, the appreciation of the Society is due the following members who represented it at various functions:

Leo G. Reeder at Wisconsin Association for Better Radio and Television Coordination Council for Better Broadcasting.

Madge Stewart Sanmann at Monmouth College Centennial Convocation and Inauguration of Robert Wesson Gibson as President of the College.

Nelson Foote at Conference on a Federal Department of Welfare.

Herbert Blumer at inauguration of Clark Kerr, Chancellor of University of California, Berkeley.

Leonard Broom at inauguration of Ray B. Allen, Chancellor of University of California, Los Angeles

Arthur L. Beeley at inauguration of J. Richard Palmer, President of Westminster College

Wellman J. Warner at Inauguration of Buell Gordon Gallagher, President of City College of New York.

A. Warren Stearns at Centennial Celebration of Tufts College.

Peter Lejins at Seventh National Conference on Citizenship.

Ray E. Baber, at formal opening of Honnold Library for the Associated Colleges at Claremont, California.

Alexander Vucinich at inauguration of John Thomas Wahlquist, President of San Jose State College.

Mozell C. Hill at inauguration of John Henry Lewis, President of Morris Brown College.

Conrad Taeuber at Medina Centennial Celebration, Washington, D. C.

James H. Peeling at inauguration of Russell Jay Humbert, President of DePauw University.

Charles E. Gehlke at inauguration of G. Brooks Earnest, President of Fenn College.

Your Secretary, in closing, continues to serve as the Society's minor diplomatic official and wishes to reassure the membership that all major problems have been referred to the appropriate committees for policy decision.

Respectfully submitted,
JOHN W. RILEY, JR.
Secretary

REPORT OF THE EDITOR

The volume of manuscripts submitted to the *Review* continued to increase during the past year. For the period from June 1, 1952 to May 31, 1953 there were submitted 266 manuscripts—42 more than in the previous year. Of these, 84 were accepted and 173 rejected. It goes without saying that much worthy material is contained in the manuscripts for which the *Review* had no space. Not all of these papers were lost to the readers, however, for many were printed in other journals.

The expansion of the Editorial Board helped to accommodate the increased flow of material, but, since some members are ill, busy, or traveling, at almost any time, the size of the Board is still barely adequate, and may have to be further increased before long. The Society

owes a debt to the skilled, conscientious, uncelebrated services of the Assistant Editors.

Printing costs have been increased again this year, putting another burden on the budget. This has been partly compensated by such economies as omission of some expensive kinds of material—tables, mathematical symbols, and the like, and by ruthless restriction of authors' alterations in proof.

The most conspicuous change during the year has been the alteration of the schedule to have the *Review* appear at the beginning of the month of issue, rather than at the end. This meant the assembling, editing, and printing of the December and February issues a month apart—an effort which the editor would not care to repeat.

The Book Review department, under the editorship of S. Frank Miyamoto, continued to operate efficiently. Such controversial incidents as arise now and then prove that there is lively interest in this part of the *Review*.

The Editor expresses appreciation to the many persons who have labored for the *Review*, and to those friends who have given advice and suggestions.

Respectfully submitted,
ROBERT E. L. FARIS
Editor

REPORT OF THE EXECUTIVE OFFICER

During the past year, the Society has continued to engage in a wide variety of activities. Most of these are reported below by the several committee chairmen, but the Executive Office, in its role of facilitating agent for the Society's committees, has introduced certain innovations. For example, the 39-page Census of Research, as prepared by the Research Committee, has been mimeographed and distributed. This Census was used in the preparation of the Annual Meeting program, and in addition, nearly 300 copies were sold. It was promoted to key persons outside the membership; and it is of interest to note that copies have been purchased by such organizations as: Standard Oil of New Jersey, National Broadcasting Company, Ford Motor Company, Research Institute of America, Metropolitan Life Insurance Company, and many others.

The expanded work of the Program Committee, to take another instance, has also been supported by the Executive Office. This expansion is indicated by the fact that this year's program lists 289 participants and 170 papers. In conjunction with the program, the abstracts

of papers have been mimeographed and put together in an 86-page document. Copies have been distributed to all registrants at these meetings. Additional copies are available at \$1.00, and over 100 advance orders have been received.

Another important activity this year has been the preparation—under the direction of Miriam Alpert of the Executive Office—of a revised edition of the Directory of Members. This contains 4,250 names, 96 pages. Some 250 advance orders have been received from members. This Directory will also be made available to deans of institutions of higher learning and to potential employers of sociologists in key positions in government and industry.

As an economy measure, the number of issues of the Employment Bulletin was reduced to six this year, bringing the total number of issues to 30. Despite this reduction, 47 vacancies and 178 applications were listed. Approximately 1,150 employment inquiries were forwarded by the Office as a result. In line with past experience, approximately 10 replies were addressed to vacancies to every one addressed to applicants. While the office is in no position to know the outcome of this service in terms of actual placements, the many communications received continue to indicate that the Employment Bulletin fills a real professional need.

Two journals, *Sociometry* and *Current Sociology* have been added to the list available to members at reduced rates.

The continued activities of the Society have received the firm support of the membership despite the increase in dues. The rate of member renewal, which was 85% and 82% respectively in the two previous years, is 83% in the current year. The acquisitions of new members have declined little as compared with last year. Members added in the first eight months were 1,013 in 1950, 818 in 1951, 549 in 1952, and 518 in 1953. It should be noted that most of the new members this year have been added during May and June, coincident with the closing date for the Directory, and that the accretions are heavily among students. (69 Active, 88 Associate, and 361 Student). Total members on August 1 were 3,875 in 1951, 3,960 in 1952, and 4,027 this year. Non-member subscribers to the *Review* were 1,637 a year ago, and 1,698 this year.

Despite the uncertain financial picture a year ago, the Society ended the fiscal year 1952 with a balanced budget, as published in detail in the *Review*. Although printing costs continue to mount, the major expenditure for *Review* publication has been held to a minimum through economies introduced by the Editorial office.

Thus, the financial situation this year promises solvency, without foundation support, and at a high level of activity.

Respectfully submitted,
MATILDA WHITE RILEY
Executive Officer

REPORT OF THE MEMBERSHIP COMMITTEE

It is now clear that the expansion of the Society during the postwar period represented stable growth. The record of the past year is set in the perspective of that growth and suggests possible goals in the period immediately ahead. The Record:

As of July first there were 4027 members on the Society's roster. Last year's figure was reported as of August 1st. Since then publication schedules have been advanced and the compilation of statistics advanced to July 1st. Interpolating last year's figures, therefore, there were 3911 members on the roles on July 1st, 1952. The net increase therefore is 116.

A breakdown of the accessions figures is instructive:

Membership Accessions, Jan.-Aug.					
	1952		1953		
Active	47	9%	69	13%	
Associate	133	24%	88	17%	
Student	369	67%	361	70%	
TOTALS	549		518		

There were 448 new members added in the first eight months of this year and 549 accessions in the same period of 1952. The current year's figures will be increased, however, by the larger number of memberships received during July and August. During 1953, the proportion of accessions in the active category increased from 9% to 15%; student memberships constituted the same percentage in both years; and the associate category dropped from 24% to 18%, with 54 fewer persons from associate fields joining the Society this year.

The steady but slower expansion of the Society also exhibits a stable pattern in the composition of the membership. The following table indicates the membership growth and distribution by categories for the past three years:

Membership Distribution by Classification						
	January 1951		August 1952		July 1953	
	No.	%	No.	%	No.	%
Active	1649	46	1685	42½	1780	44
Associate	681	19	895	22½	934	23
Student	1292	35	1380	35	1313	33
Totals	3622	100	3960	100	4027	100

The core category of active members which constituted 46% of the whole three years ago and 42½% last year, rose to 44% this year. On the basis of a series of checks of departmental staff lists, catalogs and selected mailings, it is the judgment of your Committee that there is little further present expansion possible from those persons in academic affiliation. The associate category made up 19% of the membership in 1951, 22½% last year and more than 23% this year. An indeterminate increase at this point is probably possible and it is probably linked with the steadily growing use of Sociology in applied areas. The proportion of student members was 35% in both 1951 and 1952, but dropped to 32½% this year. On evidence indicated below, it is clear that this figure could be considerably increased among graduate students if the educational and professional values of membership to students were more fully emphasized by teaching staffs. The overall picture is good, indicating a Society strength of between four and five thousand in the years at hand, and with the composition by categories remarkably stable.

Loss by withdrawals. One sensitive area is always the loss of members by withdrawals or delinquency in dues payment. Last year on August 1st there were 761 members in arrears, or 19% of the membership. The picture is slightly better this year with 18% in arrears. On the other hand, our active and associate memberships have held up better than the student memberships which normally constitute the largest single category of non-renewals. This last is a matter so largely dependent upon the co-operation of departmental staffs that this is a suitable place to call to the attention of the Society a study of the extent of graduate education in Sociology, both because of its bearing upon membership statistics and its interest to the profession.

Graduate school enrollment. In April your Committee wrote to all the chairmen of departments of Sociology in this country where graduate degrees are offered. The Federal Office of Higher Education lists 107 such institutions. In a show of fine cooperation, more than fifty departments responded and forty-nine of them sent in the names and addresses of all their graduate students. This included nearly all of the larger graduate centers. The lists were then checked against our membership files. This therefore gives us not only a picture of the potential over against the actual student memberships, but also a reasonably accurate view of the present recruitment resources to the profession.

In the forty-nine reporting institutions there were enrolled 1610 graduate students. The aver-

age was 33 graduate students for each department, but the actual enrollment ranged from 1 to 219. Five departments provided nearly 40% of the total enrollment. Nine departments each with an enrollment of sixty or more totalled 889, making up 55% of the total reported. The following table indicates the distribution:

Size of Graduate Enrollment in 49 Institutions

Size of Department	No. of Departments	Enrollment	% of Total
100 or more	3	471	30%
80-99	2	164	10%
60-79	4	254	16%
40-59	4	187	12%
20-39	11	314	19%
1-19	25	220	13%
	49	1610	100

An examination of the non-reporting departments, distributed by category, leads to an estimated additional enrollment of 800. This would indicate a total graduate enrollment in Sociology in this country for 1952-53 of approximately 2400.

Membership-wise, this points up the prospects for future active members. For the present all of these are eligible for student membership. A check of our roster reveals that of the 1610 enrollees listed only 581, or 36%, are members. If the same ratio is applied to the estimated total of 2400, there are more than 1500 graduate students in our departments who are not members. If departmental chairmen will cooperate by sending us the names and addresses of their graduate students each year, our experience indicates that a large part of the total will begin to get the habit of supporting the Society. Incidentally, it may be worth noting that in the two largest departmental enrollments, 40% and 47% respectively, were members; the average for the nine largest was 36%; and the average for the 25 departments in the 1-19 category was 35%.

Operations:

The work of your Committee has been maintained throughout the year and has been implemented by the fine support of the efficient Executive Office team. Periodic requests upon and reports to the 257 representatives of the Committee have been made, in addition to much personal correspondence. The Executive Office staff has, in addition to sending out more than eight hundred student membership invitations, made eleven mailings to potential active and associate members. In the aggregate this represents 1955 letters, from which 148 member-

ships resulted. Two of these mailings were to members of affiliated regional societies, 131 of whom were not members of the American Sociological Society. Since only six applications were returned, it is clear that there are many individuals in the affiliate societies who do not support the parent organization.

The prospect:

The membership condition of the Society is a strong and healthy one. But even with the utmost economy, the present high level of costs emphasizes the importance of continued sound growth of the Society as the condition of maintaining its present services to the profession. As specified above, there is ample opportunity for a steady growth in all three categories.

Again, your chairman wishes to express thanks for the help of members and representatives of the Committee, and especially to register his continuing appreciation of the splendid team-work of the staff of the Executive Office.

Respectfully submitted,
WELLMAN J. WARNER,
Chairman

REPORT OF THE COMMITTEE FOR LIAISON WITH THE NATIONAL COUNCIL FOR THE SOCIAL STUDIES

The purpose of the Committee of the American Sociological Society for Liaison with the National Council for the Social Studies is to improve the recognition of the interrelationships between the science of sociology and the social studies as taught in the schools.

Pursuant to this accepted purpose, the following has been done since the 1952 meetings of the American Sociological Society:

1. Scheduled a series of meetings as follows:
 - a. Meeting in Dallas, Texas, November 28, 1952 in connection with the meetings of the National Council for the Social Studies.
 - b. Open meeting to be held at Berkeley, August 30, 1953.
 - c. Luncheon meeting in connection with the National Council meetings in Buffalo, New York, November 27, 1953.
 - d. Arranged for a section meeting on Educational Sociology for the 1953 Berkeley meetings.
2. Worthy of note are the following aspects of the continuing work of the Committee which are introduced at the open meeting scheduled for these meetings August 30, 1953:
 - a. Comments on the possible contributions of sociology to the social studies by two outstanding high school teachers in the San Francisco Bay Area, John R. Carr, San Francisco, and Helen J. Mitchell, Oakland.

- b. Initiation of studies of sociological concepts held by social studies teachers by Stanley P. Wronski.
- c. The beginnings of a study on the civic attitudes of teachers supervised by C. G. Swanson.
- d. Analysis of sociological knowledge in certain publications of the National Council for the Social Studies by Wallis Beasley.
- e. Preparation of an outline for a possible booklet for teachers of the social studies entitled *Sociology in the Social Studies* being developed by Leslie D. Zeleny and members of the Committee and the publications committee of the NCSS.

It is recommended that this Committee be continued by the American Sociological Society.

Respectfully submitted,
 Wallis Beasley
 Claude C. Bowman
 Harrington C. Brearley
 Clark W. Cell
 Joseph B. Gittler
 Leo A. Haak
 C. G. Swanson
 Stanley P. Wronski
 Leslie D. Zeleny
Chairman

REPORT OF THE COMMITTEE ON STANDARDS AND ETHICS IN RESEARCH PRACTICE

Sociology finds itself in a position of continually increasing importance and also, as a part of this, of greater and greater exposure to criticism and even attack. It is because of this, as well as of other considerations, that the Society asked this Committee (instituted in 1951 as the successor to the Committee on the Problems of the Individual Researcher) to continue to function in 1952-1953 with an expanded and more representative membership.

After discussing areas of ethical concern with President Samuel A. Stouffer and Executive Officer Matilda White Riley, it appeared to the chairman that the following *relationships* should be given especial consideration this year by the committee:

1. Those of private non-profit institutions with sources of research funds.
2. Those of public institutions with sources of research funds.
3. Those of individual researchers, including textbook writers, with both subtle and crude outside pressures.

In addition, a fourth area is pressing which is indicated by this question:

4. What types of research projects, with and

without subsidies, are proper for researchers to undertake (a) as part of an academic career, (b) in an academic research bureau, (c) in an independent non-profit research institution, (d) in a commercial research agency, and (e) in a research department of a government, civic organization, or business?

Since the American Association for Public Opinion Research and the American Marketing Association deal with the major ethical problems of commercial research organizations, little emphasis is given to this area in our present list.

The chairman made available to the new committee the results of the predecessor committee's discussions and then arrangements were made for individual committee members to undertake to write memoranda on each of the problems listed above. Those who wrote memoranda on the topics listed are, in order, the following: (1) Hans Zeisel, (2) Carroll D. Clark, (3) Mabel A. Elliott, and (4) S. Michael Miller. In addition, Glaister A. Elmer also developed a useful paper on "Integrity: The First of the Field Research Requirements" which was made available to the Committee.

These memoranda were circulated to the Committee members, and many members then wrote comments on them. The memoranda are being used in two symposia, one on "Professional Responsibilities" at the 1953 Liege Congress of the International Sociological Association and the other the open meeting of this Committee at the Berkeley convention of the Society. The memoranda, as adapted in discussion, are being made available to the *American Sociological Review*.

Your Committee has visualized its role at this stage primarily as a stimulator of discussion on standards and ethics in sociological research. The need for enlightened consensus is great, but it must be admitted that sociologists lag far behind the physical and biological scientists and especially behind the psychologists in the serious attention they have given this field. Hence, the Committee's chief contributions this year are the memoranda it has developed under the guidance of certain of its members.

Your Committee wishes to call especial attention to the publication in 1953 by the American Psychological Association of the 171-page report of its Committee on Ethical Standards. This volume developed from memoranda and reports which appeared over several years in *The American Psychologist*. The problems in ethics and standards confronting the psychol-

ogists are probably less* pressing than those facing the sociologists, but the A.P.A. Committee has made a signal contribution far ahead of anything your Committee was authorized or equipped to undertake.

Your Committee urges that the Society appoint a new Committee on Standards and Ethics in Research Practice to give especial attention to (a) the rights and needs of graduate students and (b) competition between academic and commercial research agencies. It also urges that the new Committee be given cooperation both by the editors of the *American Sociological Review* and by the program committee of the Society so that its role as stimulator of discussion on pressing issues in this area may have better opportunities for implementation. It also believes that the stage is being reached at which tentative formulations of official attitudes toward standards and ethics in research can be undertaken. These should not be drafted as efforts at "legislating morals" but rather as efforts to crystallize and give enlightened direction to the evolving consensus.

Ray H. Abrams

** Bernard Barber
Gordon W. Blackwell
Herbert Blumer
Carroll D. Clark
Mabel A. Elliott
Glaister A. Elmer

** Nelson N. Foote
Robert N. Ford
S. Michael Miller
Hans Zeisel
Alfred McClung Lee
Chairman

REPORT OF THE COMMITTEE ON RELATIONS WITH SOCIOLOGISTS OF OTHER COUNTRIES

The Committee has had no meetings but has attempted to carry out its mission by correspondence and individual effort. One of the tasks has been to give all possible aid to the Second World Congress of Sociology being held in Leige in 1953. Some 30 Americans are attending and will be making important contributions. A second concern has been to aid UNESCO and the International Sociological Society in securing adequate coverage of American materials in the international quarterly, *Current Sociology*. Finally, members of the

*Two members of the Committee said this word should more accurately be "more."

**No word of approval or disapproval from these two before announced deadline date.

Committee have helped in securing new foreign members for the American Sociological Society.

Respectfully submitted,

ROBERT C. ANGELL
Chairman

REPORT OF THE COMMITTEE ON RESEARCH

Census questionnaires returned by the membership before January 20, 1953 constitute the principal basis for this report. Our activity as a committee during the past year has been directed primarily to the creation of a reporting procedure which would to a maximum degree

- 1) help the individual sociologist become familiar with work in process;
- 2) serve the needs of regional and national program chairmen; and
- 3) improve the ability of the Executive Office to respond to requests from other agencies about the operations of the Society.

We feel that with the help of the Executive Office we have made satisfactory progress on points 2 and 3. If national programs continue to be arranged so that members who have requested time to present their work will be accommodated, we believe the census may continue to have a central role in this procedure. If the census is not to be used for making up the national programs, the third objective would remain, but we would probably have to greatly increase the value of the Census to individual sociologists if the presently required expenditure of energy were to be justified. Thus far, less than 300 copies of the Census have been sold to individual members. For the coming year we hope that it will be possible to improve the description of the projects and we propose that further evaluation of the value of the Census to individual sociologists be deferred until we have one more year's experience.

In the past, the report of the Committee on Research has placed emphasis on the number of research reports received and the distribution of the projects into fields. A comparison of our present returns with those of an earlier period may be confusing. The current Census followed closely in the year the questionnaire from the previous year, so some persons may have felt that they had already reported, and our request may have been interpreted by some as placing greater emphasis on empirical projects which could be reported on at the Berkeley meetings. Table 1 reflects the decrease from 779 projects in 1949 to 641 in 1953.

Concerning the assignment of projects to fields, more than fifty percent of the projects

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TABLE 1. CENSUS PROJECTS BY FIELDS
1949 AND 1953

Field	1953	1949
Social Psychology	61	116
Population	49	45
Industrial and Occupational Sociology	48	56
Marriage and Family	46	77
Social Organization	36	10
Urban Sociology and Ecology	36	34
Methods of Research	32	31
Political Sociology	30	24
Cultural Sociology and Anthropology	28	12
Public Opinion	28	36
Community	27	46
Criminology	26	38
Race and Ethnic Relations	24	36
Social Psychiatry	21	—
Medical Sociology	20	—
Rural Sociology	20	32
Social Theory and History of Ideas	19	64
Aging and Retirement	15	—
Small Group Analysis	15	—
Educational Sociology	14	27
Social Change	14	34
Sociology and Religion	13	27
Social Pathology	10	7
Social Welfare	9	14
Other	—	13
Total	641	779

reported were designated as pertaining to more than one field. The tabulation we show is for "first choice" only. In cases where no first choice was indicated the placement reflects the judgment of the tabulator. Compared with the 1952 report (not shown) there has been little change in the distribution, but there appears to have been a conspicuous decline in Social Theory and the History of Ideas since 1949, and moderate declines in Marriage and Family, Community, and Social Change. Social Psychiatry and Medical Sociology represent new categories which are growing rapidly. It is hoped

that better criteria for classification of projects may be developed for later censuses.

The budget for research reported in the Census in which sociologists have major responsibilities involves 262 professional and 140 non-professional man-years and more than a million dollars in 1953.

Interest has also been expressed in the source of funds for sociological research. It may be seen from Table 2 that the government and foundations account for an overwhelming portion of the support.

In an additional exploratory item we inquired: Do you believe that the research needs of the membership would be served by the Research Committee if specialists in various fields (e.g., demographers, statisticians, criminologists, area experts, etc.) were available for consultation at the annual meetings?

The responses were as follows:

	Number	Per cent
Yes	396	50
No	65	8
No opinion	153	20
No answer	171	22

From the 396 "yes" responses, there were 170 persons who specified the particular type of specialist they desired to consult. Persons who receive no support for their research more frequently wished to consult specialists, and respondents from the Mountain and South Atlantic regions express a somewhat greater interest than the membership as a whole.

Insofar as it is the responsibility of the Committee on Research to work generally to promote the research activities of the society, we feel it is appropriate to call to the attention of the membership and the Council our finding that there are more than 150 members of the Society who believe their research activities would be facilitated if they had the opportunity to consult specialists of the type who are reg-

TABLE 2. SOURCE OF SUPPORT AND EXTENT OF RESEARCH ACTIVITY

Source of Support	Number	Man-Years	Amount	Man-Years per Project	Amount per Project
University	79	29	\$ 42,000	.37	\$ 532
Foundation	88	71	319,000	.81	3,625
Government	111	129	573,600	1.16	5,168
Other (including Industry)	35	41	51,350	1.17	1,467
University and Foundation	34	48	140,000	1.41	4,138
University and Government	19	14	58,850	0.74	3,097
University and Other	3	2	35,000	0.67	11,667
Foundation and Government	12	17	88,500	1.42	7,375
Foundation and Other	5	2	0	0.40	0
Government and Other	2	1	0	0.50	0

ularly in attendance at the annual meetings. We raise this point without recommendation because there is some doubt as to whether the commitments on the time of members at the annual meetings could be sufficiently reduced so that more consultations would be practicable. Informal consultations are, of course, always taking place and are, in the thinking of many, a primary justification of the meetings. The question we raise for further consideration pertains only to whether the Society might by some device facilitate such exchanges in instances where the informal process is now not adequate.

Respectfully submitted,

Paul C. Glick
Sigurd Johansen
Paul Meadows
Daniel O. Price
H. Ashley Weeks
Arthur L. Wood
Fred L. Strodbeck, *Chairman*

REPORT OF THE COMMITTEE ON TRAINING AND PROFESSIONAL STANDARDS

Two years ago the Committee on Training and Professional Standards was created. The purpose of this Committee, as specified by the 1950 Reorganization Committee of the Society, is to "continuously investigate and consider the question of standards for the profession as a whole." Furthermore, "the Committee should begin by studying current standards of professional training and research, with a view to the later development of recommended minimal standards." (*American Sociological Review*, Vol. 15 (August, 1950), p. 562.)

The present membership of the Committee, which was elected by the Council in September, 1952, consists of Harry Alpert, Joseph P. Fitzpatrick, John Foskett, Ruby Jo Kennedy, Elio D. Monachesi, Meyer Nimkoff, E. William Noland, and Calvin F. Schmid, Chairman.

The following is a summary of the work of the Committee during the past twelve months:

(1) The opinions and comments expressed by 31 sociologists to an invitation sent out by the Committee in January, 1952, to indicate the major shortcomings which seem to exist in the training of professional sociologists at the Ph.D. degree level, together with considered suggestions for improvements, were edited and summarized in a 21-page report. Copies of this report are available for distribution by writing to the Chairman.

(2) With a view of obtaining possible leads

and suggestions for a survey of graduate training and standards for sociologists, the following reports were examined:

(a) Lloyd E. Blauch and George L. Webster, *The Pharmaceutical Curriculum*, 1952.

(b) Edward C. Elliott, Director, *The General Report of the Pharmaceutical Survey, 1946-1949*, 1952.

(c) Ernest V. Hollis and Alice L. Taylor, *Social Work Education in the United States: The report of a study made for the National Council on Social Work Education*, 1951.

(3) Correspondence was carried on with Professor Frederick J. Adams of M.I.T., who is conducting a study of professional education in the field of city and regional planning under the auspices of the Alfred Bettman Foundation, in cooperation with the American Institute of Planners and American Society of Planning Officials.

(4) Following correspondence with Dr. John H. Fisher of the Modern Language Association concerning the general problem of teacher training, the Chairman was invited to attend a conference on September 25 and 26, 1953, in Washington, D. C., sponsored by the American Council of Learned Societies to explore "what learned societies can do to insure adequate standards of teacher recruitment and training and better instruction in their respective subjects at every level in our schools and colleges."

(5) The work of the Committee during the past two years, including its discussions, correspondence, and review of studies, definitely points to a need for a careful and systematic survey of various problems relating to training and professional standards of sociologists. Any useful work of this kind will require adequate funds. This fact has been recognized by professional organizations that have completed studies of curricula, training programs, standards, and related problems. The Chairman has written informally to Dr. Elbridge Sibley of the S.S.R.C. for advice and suggestions concerning possible sources of funds for a study by the present Committee.

The Committee recommends that its successor devote the coming year in formulating a prospectus and in developing a specific research design for a Survey of Training and Professional Standards in Sociology with a view to seeking financial support from some outside source in the near future for conducting such a survey. The Committee feels that the time is ripe to ascertain in a thorough and objective manner what kind of training professional sociologists receive, and on the basis of a careful evaluation of the facts, formulate a program for

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improvement. A survey such as has been proposed may be regarded as a comprehensive fact-finding phase of planning for the progressive development of the profession of sociology.

Respectfully submitted,
CALVIN F. SCHMID
Chairman

REPORT OF THE PROGRAM COMMITTEE

This is the report of the experiences of the Program Committee for the 1953 meeting. A number of innovations were attempted this year and therefore the experience of this committee may be more than ordinarily useful for the guidance of future committees.

President Stouffer directed that the program was to be made up largely of short papers, limited to 1200 words of verbal presentation, and emphasizing reports of research or research-relevant material. The papers were to be solicited from the entire membership. In effect this directive made the whole program a program of contributed papers, and it was hoped that this would result in a much broader participation by the membership than had been the case in the past.

Two main methods of soliciting papers were employed:

(1) A form letter was circulated to the membership inviting the submission of one-page proposals on the basis of which the Program Committee could judge suitability for presentation or perhaps suggest changes. Approximately 150 abstracts were received. Almost all of them either proposed something in an acceptable form or contained elements which could be modified to warrant the submission of a paper. Approximately two-thirds of the abstracts "accepted" eventually yielded papers.

(2) From the Census of Research requests for papers were sent to more than 200 members who indicated that a research project completed or in progress would yield a suitable paper in time for screening and that other publication had not yet been arranged. The use of the Census for this purpose was hampered by incomplete or ambiguous answers to the Census of Research. From this source almost all of the residue of the papers was derived.

Our experience suggests the following changes for later committees which may have the same sort of directive:

1. A slight increase (perhaps to 1500 words, but certainly not more than 1800 words) may be a more suitable length for the reporting of

sociological research at meetings. Many members felt under great pressure because of the 1200-word limit. The wisdom, however, of specifying a limit by the number of words rather than by the number of minutes allowed for presentation cannot be denied, and the control of a large program is greatly facilitated by this means.

2. The soliciting of abstracts had mixed results. Undoubtedly some members were encouraged to formulate in a preliminary way work which was not yet sufficiently advanced for a completed paper, and to the extent that that occurred the experiment was justified. On the other hand, the task may have been regarded as an additional burdensome job and may have impeded the preparation of some papers. A surprising number of approved abstracts never yielded completed papers.

3. As already indicated there was not a fair test of the usefulness of the Census for inviting papers.

4. Programs with heavy emphasis on short research reports might benefit from daily sessions in which (thematic) topics of general interest received more extended discussion.

5. More important than any of the foregoing was the fact that despite the efforts of the Executive Office to inform the membership about the new procedures, many individuals assumed that in due course they would be invited to give papers as they had in previous years. Indeed, some who submitted proposals or responded to Census invitations indicated that they would prefer a "regular" paper. This clearly revealed what many of us in the Society have long suspected, that the presentation of a contributed paper has an invidious connotation and may be risked only by the young, the little known, or the careless. If the experience of this committee can benefit the Society only in one way, we urge that never again shall the Society have a segregated section of contributed papers. We further advise sustained efforts to solicit papers from the broadest representation of the membership.

It is our judgment that the expanded participation represented in this program is a token of at least a partial success of some of the policies.

The Chairman would like to express his thanks to the President, the Executive Office, and the several members of the committee who worked diligently, with good judgment, and with devotion to the Society.

Respectfully submitted,
LEONARD BROOM

REPORT OF THE COMMITTEE ON RELATIONSHIP BETWEEN THE SOCIAL SCIENCES AND THE AAAS

This *ad hoc* committee was established at the Society's 1952 meeting in response to considerable questioning of the value of our formal participation in the Association's affairs. The chairman was requested to discuss the matter with other social science societies and prepare a recommendation for the Society's 1953 meeting. Due to the fortunate coincident that Drs. Nelson and Taeuber are the executive officers of Section K and members of the AAAS Council, the chairman worked with them on the problem and is reporting joint conclusions.

The recommendation is that action by the Society be deferred until the 1954 meeting because of the Association's current move to redefine its objectives and program, and reorganize its permanent staff. In our opinion, the Society should wait until the smoke clears a bit so that a decision can be based on more solid ground.

For some time there has apparently been concern over the basic policy and program of the Association among some scientists, eventuating in a meeting of the AAAS Executive Committee in September, 1951. The recommendations of this so-called "Arden House Conference" were summarized as follows:

- (1) A real strengthening of the direct usefulness of the AAAS to scientists and to scientific societies.
- (2) A shift in emphasis from the more detailed aspects of the various technical branches of science to the broader problems of science as a whole.
- (3) The cultivation of synthesizing and unifying activities as the main emphasis of the AAAS in its internal work within the body of science.
- (4) The undertaking of attempts to improve public understanding of science as the main external emphasis of the AAAS.

Since this Conference report appeals to Drs. Nelson and Taeuber as well as to the undersigned as a desirable and promising redirection of the Association, and since the new management of the Association appears to be intent on having it adopted, it would seem to be advisable at this time to continue the Society's relationship with the Association unchanged.

Respectfully submitted,
RAYMOND V. BOWERS
Chairman

REPORT OF THE COMMITTEE ON THE BRITT AWARD

In April 1953, the Executive Office circularized information about a grant-in-aid award for a research project in social psychology made possible by the Britt Foundation.

Due to the lateness of the announcement there was little time to secure wide publicity and response. Nevertheless, seven applications representing a wide distribution were received by the closing date, June 1.

The Committee has carefully considered these applications and has named as the selected research project the application of Murray A. Straus, Research Assistant at the University of Wisconsin, for a project entitled "Child Training and Child Personality in a Rural and Urban Area of Ceylon".

Respectfully submitted,
EDGAR A. BORGATTA
WILLIAM J. GOODE
WELLMAN J. WARNER, *Chairman*

REPORT OF THE REPRESENTA- TIVE TO THE AMERICAN AS- SOCIATION FOR THE ADVANCEMENT OF SCIENCE

At the St. Louis meeting of the AAAS, December 27-30, 1952, Dr. Lowry Nelson was elected Chairman of Section K (Social and Economic Sciences), succeeding Dr. Howard R. Tolley. Dr. Conrad Taeuber continues as Secretary. Dr. Edward U. Condon, formerly Director of the National Bureau of Standards and presently of the Corning Glass Works, is President of the Association for 1953, and Dr. Warren Weaver of the Rockefeller Foundation was elected to succeed him in 1954.

Among the programs of Section K at the St. Louis meeting, the following were of particular interest to sociologists: a Symposium on the Social Sciences and Humanities in Medical Education (jointly with Alpha Epsilon Delta); a series of symposia on technical assistance programs (jointly with Section H—Anthropology); and a Symposium on Information Processing in Social and Industrial Research (jointly with Section P—Industrial Science, the National Academy of Economics and Political Science, and Pi Gamma Mu).

From the minutes of the AAAS Council's meetings, the following items should be reported: a revised constitution and by-laws were approved (see *Science*, Nov. 21, 1952, pp. 575-578); negotiations are still going on with reference to the building of a new headquarters in

Washington, D. C.; the Association has been actively concerned with certain provisions of the McCarran Act of 1950 and the McCarran-Walter Act of 1952; and the schedule of future meetings includes Boston (1953), San Francisco (1954), Chicago (1955) and New York (1956).

Section K has 851 members, of whom 242 are listed in the Directory of the American Sociological Society. This is the best estimate we have of the profession's interest in the Association, although it might also be pointed out that five of the present twelve members of the Section K committee are sociologists.

The 1952 \$1,000 AAAS Prize in the Social Sciences, first offered in 1951, was awarded to Dr. Arnold Rose. Dr. Stuart Dodd received Honorable Mention. The contest for 1953 has been suspended.

Responsibility for the program of Section K at the 1954 San Francisco meeting has been delegated to a West Coast group of whom Dr. Carlo Lastrucci of the University of California is the sociologist.

Respectfully submitted,
RAYMOND V. BOWERS

REPORT OF THE REPRESENTATIVE TO THE AMERICAN DOCUMENTATION INSTITUTE

I submit herewith my report for the year 1952-1953 as the Society's representative in the American Documentation Institute. On September 26, 1952, I attended the only meeting of the Institute at which I have been able to be present during my two years as a member, all other meetings during that period having fallen on days when I was out of the city or previously committed to other engagements. I offer my apologies for having been unable more actively to represent the Society but I frankly feel that no substantial loss has resulted, for I have been unable to see that either the Institute's current activities or those of the Society have been in any way handicapped as a consequence.

The Institute's activities are of two types. It serves as a repository for documents of which it supplies microfilms or photoprints at stated prices, and it publishes a journal devoted largely to information concerning mechanical aids for documentalists.

The Institute, whose membership was originally limited to representatives nominated by learned societies, library organizations, and the like, was opened a year or more ago to individual membership. This change was ardently supported by some of the original members and bitterly opposed by some of the others. I do not

believe that this controversy is one in which the Sociological Society has any interest. In its earlier years I believe that the Institute performed a useful function in fostering the use of microfilm and other devices for the preservation and reproduction of documentary materials which could not have been economically printed. At present the Institute seems to me to have become an organization of technicians—chiefly librarians—with no special interest in sociology or in social sciences in general.

Should the Society wish to continue its affiliation with the Institute I would respectfully request that I be replaced by some representative with an active personal interest in problems of documentation and particularly in mechanical aids to the processing and reproduction of documents.

Respectfully submitted,
ELBRIDGE SIBLEY

REPORT OF THE DELEGATE TO THE AMERICAN COUNCIL OF LEARNED SOCIETIES

The annual meeting this year was marked by a very interesting innovation of four round-table discussions. The topics were the nature of the Humanities, their function in our society, and plans for furthering the work of the ACLS.

A background paper entitled "The Humanities in the Next Decade" had been prepared in advance by the Executive Offices and circulated before the annual meeting. This paper served the admirable function of stimulating discussion without controlling it. This delegate was in the round table chaired by Dr. Henri Peyre and found the discussion highly stimulating. There was a feeling of real participation, an exciting exchange of ideas, and a sense of the seriousness of problems concerning the humanities. The same attitudes were expressed at the final session when the rapporteurs reported on their respective sessions. There was a general attempt to define the Humanities and there was agreement that a precise definition was not possible since all intellectual activities had humanistic aspects at times. It was also considered important to stress the coalescent quality of the humanities. The two-fold problem of communication—communications with fellow scholars and communication of the results of our studies to society—was stressed, particularly at the round table of which I was a member. Each round table also considered the relations between the Council and the constituent societies and there was discussion of how the membership of the constituent societies might be kept better informed of the activities of the ACLS. The

functions of the Council were among the problems discussed and were classified under the headings: Personnel, Implementation, Communication and Research . . . and excellent suggestions were made for each one.

Reports on the various committees, projects and conferences had been distributed in advance of the meeting. The resignation of Charles E. Odegard as Executive Director was an occasion for regret to the Board of Directors and to the participants. The officers for 1953-1954 are: C. W. de Kiewiet, Chairman, Roger P. McCutcheon, Vice-Chairman, William R. Parker, Secretary, Sidney Painter, Treasurer.

The meetings gave a sense of intelligent and urgent concern with Humanistic scholarship, with its relationship to other disciplines, and its broader relationship to society. This delegate urges close and active participation in the work of the ACLS.

Respectfully submitted,
HORTENSE POWDERMAKER

REPORT OF THE SENIOR REPRESENTATIVE ON THE SOCIAL SCIENCE RESEARCH COUNCIL

It is a pleasure to present to the Council this report of the year's activities of the Social Science Research Council on which I have had the very great honor of being one of your representatives during the past three years. Now in its thirtieth year—if my arithmetic is correct—the SSRC has just achieved a measure of financial stability such as it has not known in the past. I am sure the Society will be interested in knowing that the SSRC has been given a capital grant of \$1,500,000 from the Rockefeller Foundation from which only the income may be used for a period of ten years, after which both capital and income may be drawn upon if and as needed. This fund, together with the reserve which the Council has accumulated over the years, it is expected, may within a short time make it possible to bring the capital fund up to \$2,000,000. The income from this amount will enable the Council to operate on about its present level as far as administrative and planning expenses are concerned. The significant fact is that the Council will no longer, as it has in the past, be compelled to operate entirely on two- or three-year grants, but will enjoy a degree of stability and security it never has had before.

Under the Council's program for the development of research personnel, awards of two general types are made: fellowships to enable promising young scholars to undertake special train-

ing in research, and awards to aid mature scholars to carry out their own research. In recent years special summer research seminars and one training institute have also been held.

Thirty-seven research training fellowships, and twelve area research training fellowships have been awarded this year. In addition, two new training programs were inaugurated this summer. The Institute in Mathematics for Social Scientists held at Dartmouth during the summer was an outgrowth of the Council seminar held last summer for the preparation of materials for teaching the mathematics needed for research on social science problems. On invitation of the Council's Committee on the Mathematical Training of Social Scientists, 43 faculty members and graduate students were invited to attend the institute.

The other new program launched this summer provides undergraduate research stipends to enable carefully selected students to get experience in actual research under supervision of a faculty member before entering upon the senior year in college. The students receiving these grants are expected to devote about eight weeks or more during the summer to their projects and bring them to completion during their senior year. It is expected that upwards of one-half of these students may receive grants for their first year of graduate work. The undergraduate stipend is \$600, with the faculty superior receiving a similar amount. *Forty-one students and 39 faculty supervisors were chosen for the first year (1953).* Ten of the students are in the field of sociology. Funds granted to the Council by the Ford Foundation provide for continuing the program for three years. These grants clearly have a recruitment angle, as well as training. The purpose of the program is to provide experience in doing social science research and opportunity to consider it as a career, before the time when career choices are made.

Closely related to the personnel training activities is the support that the Council has been able to give to individual scholars. Faculty recipients of these awards certainly get some "training" out of them, although their basic purpose is to enable scholars to carry through projects of their own conception and in their individual interests. The Faculty Research Fellowships are designed to enable young social scientists, whose superior qualifications have been demonstrated by outstanding research to devote half or more of their time to self-directed research while remaining in residence at their universities. Six such grants were made for three-year periods beginning in the fall of 1953.

The program of grants-in-aid to mature scholars of recognized competence who do not have access to research funds within their own

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institutions is a continuing one. Of the 25 grants made for the coming year and announced in the June issue of *Items*, only one appears to be to a sociologist.

Members of the Society serve on two-thirds of the 30 odd committees of the Council. Chairmanships of six important committees are held by sociologists. These committees are: Measurement of Opinion, Attitudes, and Consumer Wants (S. A. Stouffer); Migration Differentials (Dorothy Thomas); Pacific Coast Committee on Community Studies (Leonard Broom); Social Behavior (L. S. Cottrell, Jr.); Family Research, (E. W. Burgess); Social Stratification (Paul K. Hatt, until he passed away January 6, 1953). Sociologists are interested in many other activities of the Council besides those represented by the committees mentioned, but it is appropriate to briefly indicate what these committees are doing or planning to do, and then say a word or two about the other activities of the council.

For the Committee on Family Research, Leonard S. Cottrell, Jr. and Nelson N. Foote have prepared a comprehensive planning report which incorporates suggestions for needed research on problems in marriage and family relations. The report is scheduled for publication during 1954.

The Committee on Measurement of Opinion, Attitudes and Consumer Wants has under preparation a final report on its studies of sampling methods, and has provided guidance for two studies, one by the National Opinion Research Center on isolation, measurement and control of interviewer effect, and the other by the Columbia University Bureau of Applied Social Research on panel methods.

A revision of *Research Memorandum on Migration Differentials* (1938) by Dorothy S. Thomas has been under way since 1950 and is scheduled for completion and publication in 1955.

The Pacific Coast Committee on Community Studies has been engaged in preparing a memorandum on the methodology of ordering the mass statistics of metropolitan areas.

One of the most significant efforts of the Council in carrying forward its purpose to explore new areas for research is represented in the activities of the Committee on Social Behavior, of which Leonard S. Cottrell, Jr. is chairman. Current emphasis is upon cross-cultural research possibilities in three general areas: socialization, communication and interpersonal influence, and social integration. A subcommittee on child development has just published a bibliography on the socialization of the child, prepared by Christoph M. Heinicke and

Beatrice B. Whiting. A manual for the cross-cultural study of socialization is under preparation. Three working conferences were held during the summer of 1952 on the subject of social integration, and other conferences on the problems related to communication and interpersonal influence were held for the purposes of defining crucial problems for research.

The *Committee on Cross-Cultural Education* is carrying on an exploratory program of research on the results of exchange of persons programs. The studies during the first year are concerned with students from four nations: Japan, Mexico, Sweden and India, and are being made at Ohio State University, University of California at Los Angeles, University of Wisconsin, and University of Pennsylvania.

The approach of the Committee on Identification of Talent is of special interest to sociologists. The emphasis of the committee is on the study of "nonintellective factors" which may contribute to high achievement rather than on measures of intelligence. Answers are being sought to problems of motivation, individual values, and cultural factors as related to achievement. Studies are being sponsored at Wesleyan University on "achievement motivation," at Cornell University on "social sensitivity," and at Yale University on "cultural determinants of achievement in given social situations."

The Council has continued its interest in and support for international cooperation among social scientists and the promotion of area studies and area research. There are active special committees on the Near East and Slavic countries, investigating the research and training needs in these respective areas. Representatives of the Council have attended conferences called by UNESCO to consider the organization of an international social science council. The cross-cultural education project is itself an expression of this interest in research possibilities involving countries other than our own.

Many other activities of the Council which focus primarily on other social science disciplines are of interest to sociologists, but in view of recent reports of my predecessors which have made reference to them, and also to the fact that the interested individual may find ready access to information on these points from recent issues of *Items*, it seems unnecessary for me to expand this report any further.

Again, I express my deep appreciation to my colleagues in the Society for the privilege of sharing in the deliberations of the Social Science Research Council these three years past. The organization is continuing to fulfill its purpose to "plow new ground."

Respectfully submitted,
LOWRY NELSON

NEWS AND ANNOUNCEMENTS



The next annual meeting is to be held at the University of Illinois, Urbana, Illinois, on September 8-10, 1954.

Announcement of Special Issue. The Review plans to publish a special issue on Small Groups Research, as the October, 1954 issue. Fred L. Strodbeck has consented to act as editor for this number. Papers are solicited from all who are engaged in current research in this field. They may be sent to the Editorial Offices, 116-A Smith Hall, University of Washington, Seattle 5, Washington. Selection of the papers from those submitted will be made by Professor Strodbeck.

The Luigi Sturzo Institute, Rome, Italy, founded on the 25th of November, 1951 by Decree No. 1408 of the President of the Italian Republic, is offering a prize by competition for a paper on sociology which constitutes an effective contribution to this field of study both from the point of view of serious research and maturity of thought. The paper should be free from editorial restrictions and ready to go to print.

The subject of the essay is to be "The Methodological Problems and Criteria of Sociology in the First Half of the 20th Century."

The sum of four million Italian lire will be awarded as the prize for the best paper. The prize may not be divided. It will be handed over one month from the day on which the report of the judging committee is published.

All scholars of any nationality may compete. Each competitor must submit his paper identifiable by his mark or pseudonym on the title page. The paper must be accompanied by a sealed envelope marked on the outside with the mark or pseudonym and containing the competitor's full name and address, besides a brief account of his or her scientific achievements.

The paper must reach the Secretariat of the Institute in five typewritten copies not later than April 30th, 1955 and the prize will be awarded by the 31st of December, 1955.

World Population Conference, under the auspices of the Economic and Social Council of the United Nations will be held in Rome in September, 1954. Questions related to arrangements for the attendance of participants may be addressed to the Secretary of the Sub-Committee on Arrangements, Professor Frank Lorimer, at the American University, Washington 16, D. C.

The Social Science Research Council announces the following categories of awards to be

offered in 1954: Research Training Fellowships (for predoctoral students who have completed all degree requirements except thesis; and postdoctoral students preferably not over 35 years old); Grants-in-Aid of Research (for mature social scientists, not candidates for degrees); Faculty Research Fellowships (for young faculty members who have already made significant research contributions); Undergraduate Research Stipends (for students about to complete the third year of study toward the bachelor's degree). All applicants must be permanent residents of North America.

At present, funds for Area Research Training Fellowships and Travel Grants are exhausted, and it appears likely that no further offerings of these awards can be made; but programs of study or research related to foreign areas may be presented by applicants for Research Training Fellowships or Grants-in-Aid of Research.

Applications should be sent in by January 4, 1954 to the Social Science Research Council, 726 Jackson Place, N.W., Washington 6, D. C.

Boston University. William O. Brown, formerly Chief of the Division of African Affairs in the United States State Department, has been appointed Professor of Sociology in charge of the new African Studies Program. Prior to assuming his active duties in September, Dr. Brown spent several months in Europe and Africa surveying the resources for African studies.

T. Scott Miyakawa has been granted leave of absence for the year to accept a Fulbright appointment to do research in industrial sociology in Japan.

John T. Greene has been appointed Assistant Professor of Sociology to teach courses in marriage as a replacement for Herbert D. Lamson who, after six years of effective service, has been obliged by ill health to resign. Mr. Greene is a graduate of Duke University from which he holds the degrees of B.D. and A.M. and he has completed his work for the Ph.D. degree in sociology at the University of North Carolina. For the past four years Mr. Greene has been Director of Family Life Education for the Methodist Conference in North Carolina and he has taught sections in marriage at the University of North Carolina where he has served as research assistant to Reuben Hill.

George K. Stürup, Director of the Danish institutions for psychopathic offenders at Herstedvester and Kastanianberg, Denmark, and adviser to the Danish Ministry of Justice, has joined the staff of the department on a Fulbright appointment for the first semester. Dr. Stürup will give a course in The Abnormal Offender within the framework of the department's offering in criminology.

Adelaide Hill has resumed her teaching following a semester's leave of absence to visit Africa under the auspices of the United States Department of State.

Albert Morris resumed his work in February, 1953, following his return from a Fulbright appointment to Australia and New Zealand.

University of California at Los Angeles.

Leonard Broom, Chairman of the Department of Anthropology and Sociology, attended a Social Science Research Council Interdisciplinary Symposium on Acculturation at Stanford University during the months of July and August. During the academic year, 1953-54, he will be on leave under a Faculty Fellowship from the Fund for the Advancement of Education of the Ford Foundation. He will spend the fall semester studying the racial situation in the Deep South and the spring semester in the Hawaiian Islands.

Ralph L. Beals, Professor of Anthropology and Sociology, has been participating in the cross-cultural education project of the Social Science Research Council. Under his direction, Mexican students at UCLA have been undergoing extensive interviewing concerning their adjustments to American school life.

Svend Riemer has recently been appointed to the Advisory Council of the Southern California Council on Family Relations. He continues his research on the urban environment.

W. S. Robinson returns from sabbatical leave spent in preparation of a treatise on the logic of the social science method.

Walter Goldschmidt is the recipient of a Fulbright Award and will spend 1953-54 in Uganda. He has also received a Social Science Research Council grant-in-aid.

Ralph H. Turner has received a Social Science Research Council Faculty Research Fellowship to run from September 1953 until September 1956. The award was granted on the basis of his continuing research on the personal values of upward mobile individuals.

Donald R. Cressey has been engaged in research at the Chino State Prison during 1952-53. He is attempting to develop a formula for the prediction of escapes and is examining the relationship between prisoners' conceptions of crime causation and their participation in the institutional program of the prison.

Ruth Riemer attended the Social Science Research Council seminar in advanced mathematical training during the summer of 1953. She will be on leave during the year 1953-54 for travel and advanced study in Europe.

Two new members have been added to the permanent faculty. Melville Dalton, formerly at Washington University, will be Assistant Professor of Sociology, specializing in industrial relations and sociological theory. He will spend his summers working with the Institute of Industrial Relations at UCLA. Richard T. Morris, formerly at Northwestern University, will be Assistant Professor of Sociology and will specialize in social theory and continue the work on stratification begun in collaboration with the late Paul Hatt.

Burton Robert Clark, who received the Ph.D. degree from UCLA during the summer of 1953, and who was formerly a Social Science Research Council Research Training Fellow, will be Instructor in Sociology at Stanford University.

John Itsuro Kitsuse, graduate student in the department, is the recipient of a Haynes Foundation Fellowship for the year 1953-54.

The following persons are teaching assistants in sociology for 1953-54: Herbert Louis Aarons, Jr., Helen Patricia Beem, Mary Bob Cross, Gloria Fingerhut, Sheldon L. Messinger, Channing Wayne Murray and Paul K. Rowan.

Drew University. David R. Mace, Professor of Human Relations, has been invited by the Director of Social Affairs of the South African Government to visit the Union of South Africa in the spring of 1954. The main purpose of his visit will be to help build up the marriage guidance services throughout the Union. While in South Africa, Dr. Mace will lecture at the Universities of Capetown, Stellenbosch, and Witwatersrand.

Milton M. Gordon has accepted a position as Assistant Professor of Sociology at Haverford College. Dr. Gordon is engaged in research in the fields of social stratification and ethnic group relations.

Purnell H. Benson, formerly Instructor in Sociology at Temple University, has been appointed Assistant Professor of Sociology and will be in charge of the undergraduate sociology program. He took his Ph.D. at the University of Chicago.

University of Florida. The Government of Brazil in June conferred upon T. Lynn Smith the Order of the Southern Cross. Professor Smith has spent several years in Brazil on assignment by the U. S. Department of State, as a visiting professor at the University of Brazil, and as advisor on agrarian reform to the Minister of Agriculture and the National Commission on Agrarian Policy. He is the author of several books on Brazil.

Shaw Earl Grigsby was Visiting Associate Professor of Sociology for the second semester, 1952-53. Professor Grigsby was formerly the Director of the Darmstadt Community Survey, Darmstadt, Germany, 1949-52, which was sponsored by the Office of the High Commissioner of Germany (HICOG), Foreign Service, Department of State.

John M. MacLachlan, Head of the Department of Sociology, has been on leave for one year in order to serve as Chief of Staff, Medical Center Study, University of Florida.

A Marriage and Family Clinic was established at the university under a cooperative arrangement between the Florida Center of Clinical Services and the Department of Sociology. Winston W. Ehrmann, Professor of Sociology, who has been the marriage counselor for the university since 1939, is the head and Bruce Thomason, Associate Professor of Sociology, who was formerly associated with Dr. Clifford Adams at Pennsylvania State College, is the associate in the new clinic. For the past year Professor Ehrmann has also been acting head of the Department of Sociology. He was elected chairman of the Research Section of the National Council on Family Relations for 1953.

The Florida State University. William F. Ogburn will be Visiting Professor of Sociology in the fall semester 1953-54, and will offer a graduate seminar in Social Trends.

Robert McGinnis, a candidate for the doctorate at Northwestern University, has been appointed Acting Assistant Professor of Sociology.

Howard University. E. Franklin Frazier, who has served as Chief of the Division of Applied Social Sciences of the Department of Social Sciences of UNESCO since 1951, gave a course of three lectures under the Special University Lectures in Sociology on May 4, 7 and 8 at the London School of Economics. He also lectured on sociology under the Munro Lectureship Foundation at the University of Edinburgh on May 12; and gave the Sir James G. Frazer Lecture in social anthropology at the University of Liverpool on June 26, 1953. Professor Frazier returned to his position at Howard University in September, 1953.

University of Kansas. Hilden Gibson, Professor of Sociology and Human Relations, and Chairman of the Department of Human Relations, will be on leave during 1953-54 while holding a Ford Foundation Faculty Fellowship. He will study the general education program at Colgate University and the human relations work being conducted at Harvard, Michigan, Cornell, and New York universities. During his absence, Marston M. McCluggage, Professor of Sociology and Human Relations will serve as Acting Chairman of the department.

E. Jackson Baur, Associate Professor of Sociology and Anthropology, will also be on sabbatical leave during the next academic year while devoting himself to a study of board members of welfare agencies under a research grant from Community Studies, Inc., of Kansas City, Missouri. During the past four summers he has served on the research staff of Community Studies where he has developed statistical indicators of community well-being.

Charles K. Warriner has been promoted from Instructor to Assistant Professor of Sociology and Human Relations. During the next year he will give half of his time to the direction of a research study of hospitals as social systems in Kansas City.

Carroll D. Clark, Chairman of the Department of Sociology, taught in the summer session at Washington University, St. Louis, Missouri.

Waldo W. Burchard, formerly at Denver University, has been appointed Instructor of Sociology. He is completing his doctorate at the University of California, Berkeley.

Miami University. On July 1, Warren Thompson retired as Director of the Scripps Foundation, a position which he has held since 1922. During the next several months he will be engaged in a study of the growth and changes in California's population which is being sponsored by the John Randolph Haynes and Dora Haynes Foundation of Los Angeles.

On July 1, P. K. Whelpton resigned as Director of the Population Division of the United Nations to return to the Scripps Foundation, from which he

has had a leave of absence. He is now Director of the Foundation.

Donald Bogue has been promoted from Demographer to Associate Director of the Foundation.

Michigan State College. Olen Leonard has been granted an extension of his leave for one year to permit him to continue as Director of Technical Cooperation of the Northern Zone of the Inter-American Institute of Agricultural Sciences. His headquarters are located in Havana, Cuba. Michigan State College has recently signed an agreement with the Institute to have two staff members engage in research in Latin America.

Allan Beegle has received a Fulbright award for research in Finland next year. He will study a small village community in that country in collaboration with the University of Helsinki. W. W. Schroeder, formerly a graduate student at MSC and more recently a student at the University of Chicago, will substitute in research and teaching while Mr. Beegle is on leave.

Raymond Scheele returned from Brazil at the beginning of the Spring term and has resumed his academic duties. While in Brazil Dr. Scheele made a study for the Area Research Center under contract with the United States Department of State.

J. F. Thaden will be on sabbatical leave next year. He will do research and writing in educational sociology.

The Social Research Service in cooperation with the School of Continuing Education is making a study of the social aspects of disaster. The research data are being collected at Flint, Michigan where a tornado caused serious loss of life and heavy property damage this June. The National Research Council, through the National Opinion Research Center's disaster studies specialists, is providing assistance in the study. The Social Research Service committee in charge of the project consists of the following: W. H. Form and Charles Westie (co-chairmen), and Gregory Stone.

The Department of Sociology and Anthropology through its Social Research Service is engaged in making a pilot study of one school community in order to find out what citizens know about their schools, how they secured their information, and their attitudes toward schools. This project, designated as the Michigan Communications Study, is sponsored by the Midwest Administration Center, the University of Chicago, and Michigan State College. Following the pilot study a series of follow-up studies will be made by S.R.S. to evaluate the relative effectiveness of the various methods of communications. Wilbur Brookover and Leo A. Haak are co-chairmen of the Social Research Service project committee; the latter is in charge of the pilot study. Other members of the committee are Charles P. Loomis, Sigmund Nosow, J. F. Thaden and Joel Smith.

The study of adult education in rural areas by the Social Research Service and sponsored by the Fund for Adult Education of the Ford Foundation has been completed.

Visiting instructors for the 1953 summer session included the following: Thelma R. Black, Utah Agricultural College; Richard Dewey, University of

Illinois; Dean Epley, Memphis State College; Morton B. King, Jr., University of Mississippi; Ward Porter, The University of West Virginia.

Charles P. Loomis, Head of the Department, taught in the 1953 summer session at the University of Wisconsin.

Robert A. Hicks, who is a graduate student in sociology and anthropology, has received a Fulbright research grant for the academic year 1953-54 to do research in urban ecology in the city of Baghdad, Iraq. He will be affiliated as a Research Fellow with the College of Arts and Sciences, Baghdad.

Beatrice Garner, graduate student in sociology and anthropology, received an American Council of Learned Societies Grant to study linguistics at the University of Indiana Summer Institute (1953).

Thomas L. Blair and Manuel Alers-Montalvo, candidates for the Ph.D. degree in the Department of Sociology and Anthropology, each received a 2,500 dollar Opportunity Fellowship award from the John Hay Whitney Foundation, New York, for the year 1953-54.

Five departmental seminars were held during the year with representatives of the American Universities Field Staff, Inc. The leaders for these seminars were: Albert Ravenholt and A. Doak Barnett on China; Richard D. Robinson on Turkey; Richard H. Nolte on Middle East; and Lawrence W. Witt on Brazil.

The same men participated as visiting specialists during the spring term in an inter-departmental seminar. The focus of the seminar was problems of introducing change. The cooperating departments were: Agricultural Economics, Economics, Foreign Studies, History, Political Science, Psychology, and Sociology and Anthropology. Kenneth Tiedke represented the Department of Sociology and Anthropology in this interdisciplinary project.

Mississippi State College for Women. Margaret M. Wood, Professor of Social Studies, has been awarded a Fulbright fellowship as Lecturer in Sociology at the Philippine Women's University in Manila for 1953-54.

University of New Hampshire. Charles W. Coulter, Head of the Department of Sociology, 1934-1949, and since 1949 Professor of Sociology, became emeritus June 30, 1953. He has accepted an appointment with the American Economic Foundation for the ensuing year, and has been asked to continue as a member of the State Prison Board and State Board of Parole. Dr. and Mrs. Coulter plan to spend some time in foreign travel before their return to New Hampshire.

Arthur E. Prell, who is completing his dissertation in criminology at the University of Minnesota, has been appointed Instructor in Sociology.

A. Melville Nielson, Assistant Professor of Sociology, was one of three lecturers appointed from all social science fields to participate in the senior synthesis seminar of the College of Liberal Arts.

Northwestern University. The department has added two full-time members to its staff. Raymond W. Mack came to the department in

September 1953 from the University of Mississippi. He holds the rank of Assistant Professor. Wendell Bell, now at Stanford University, has been appointed Associate Professor but will not begin his work in Evanston until September, 1954. Between them, the two men will carry forward and develop the offerings in urban sociology, stratification, and demography.

Robert F. Winch has received a two-year renewal of his grant from the National Institute of Mental Health to enable him to bring to a close his extensive research project, "A Study of the Theory of Complementary Needs in Mate Selection," an initial report on which he gave at the Berkeley meetings.

Douglas More will continue with Dr. Winch on the latter's research project and also will teach part-time in the department.

Thomas Ktsanes, who completed his doctorate in August, joined the Department of Sociology at Tulane University in September. Robert McGinnis has been appointed Assistant Professor at Florida State College. Raymond Murphy will teach at Northern Illinois State Teachers College as a substitute for Donald Roos now on leave to complete his work for the Ph.D. in this department. Arnold S. Feldman has been reappointed for 1953-54 to the staff of the Social Science Research Center, University of Puerto Rico.

Princeton University. The sociology section of the Department of Economics and Social Institutions now functions with a separate curriculum, both on the undergraduate and graduate level. A full graduate program leading to the Ph.D. in sociology is also now available.

Wilbert Moore was Visiting Professor in the Department of Social Relations at Harvard University during the summer session, 1953.

Marion J. Levy, Jr., under the terms of an SSRC grant, conducted a summer seminar on Structure of Society.

Under the auspices of the Organizational Behavior Project, of which Professor Moore is Director, a study is being conducted of the factors which differentiate the degree and quality of commitment by staff members of a mental hospital to the aim of cure and care of patients. The study is being directed by Eliot Mishler.

Also under the auspices of the Organizational Behavior Project, Gresham Sykes has been directing a study of the factors which differentiate the degree and quality of participation by parents in Parent Teachers Associations.

Melvin Tumin has received a grant from the Rockefeller Foundation for a study of the relations between occupational morale and social morale among a sample of the occupational hierarchy in Trenton, New Jersey.

Professor Tumin has also been appointed Director of the Study of Social Stratification in Puerto Rico. This study is supported by the Social Science Research Center of the University of Puerto Rico.

Frank Notestein recently delivered a series of four lectures on population problems to the War College.

Gerald Breese is actively engaged as Director of two studies in urbanization. One study, supported by the U. S. Housing and Home Finance Corporation, is investigating the impact of the location of the U. S. Steel Plant at Morrisville on the Delaware Valley area. The other, supported by the New Jersey Department of Conservation and Economic Development, is an analysis of Socio-Economic Region Number 2 in New Jersey.

Morroe Berger, formerly of Columbia University, has been appointed Assistant Professor of Sociology. He will specialize in Middle Eastern materials. Under the terms of a special grant, Professor Berger has departed for the Middle East, where, among other inquiries, he will conduct a study among the higher civil servants in Egypt.

Lloyd Fallers of the Department of Anthropology of the University of Chicago has been appointed Visiting Assistant Professor for the year 1953-54.

Eliot Mishler has been appointed Research Associate of the Office of Population Research.

The staff of the department now includes Professors Frank Notestein, Frederick Stephan and Wilbert Moore; Associate Professors Gerald Breese, Marion Levy and Melvin Tumin; Assistant Professors Morroe Berger and Lloyd Fallers; Instructor Gresham Sykes.

Inquiries regarding graduate work in sociology should be addressed to Frederick Stephan, Chairman, Sociology Section, Department of Economics and Social Institutions. Information regarding fellowships in the Office of Population Research should be directed to Frank Notestein, Director.

The Educational Testing Service is offering for 1954-55 its seventh series of research fellowships in psychometrics leading to the Ph.D. degree at Princeton University. Open to men who are acceptable to the Graduate School of the University, the two fellowships each carry a stipend of 2,500 dollars a year and are normally renewable.

Fellows will be engaged in part-time research in the general area of psychological measurement at the offices of the Educational Testing Service and will, in addition, carry a normal program of studies in the Graduate School. Competence in mathematics and psychology is a prerequisite for obtaining these fellowships. The closing date for completing applications is January 15, 1954. Information and application blanks will be available about November 1st and may be obtained from: Director of Psychometric Fellowship Program, Educational Testing Service, 20 Nassau Street, Princeton, New Jersey.

The University of Texas. Logan Wilson, formerly Professor of Sociology and Dean at Newcomb College, Tulane University, New Orleans, Louisiana, was inaugurated President of the University of Texas, Austin, Texas on October 29, 1953.

Central University of Venezuela, Caracas, Venezuela, announces the signing of a ten-year "intellectual collaboration" agreement with the University of Wisconsin which is designed to help in the expansion and modernization of the various

Divisions of the Venezuelan University System. The agreement stipulates that the University of Wisconsin will make available a specified number of senior faculty members on an annual basis and junior members on a permanent basis, and when qualified personnel are not available in Wisconsin it will attempt to arrange to procure them from other institutions. The long-range nature of the program permits the planning of staff needs three years in advance, thus giving time for designated professors to prepare themselves linguistically.

A joint committee of the two universities will be in charge of its administration with Emilio Spósito Jiménez, Secretary of the Central University, as Chairman. Associate Graduate Dean Homer J. Herriott is Chairman of the Wisconsin group, and George W. Hill will function as coordinator of the program.

Although the program envisages collaboration in all of the disciplines, the most pressing needs in the first year's operations are expected to be in the Social Sciences, Agricultural Sciences, Education, and Medical and other applied sciences. The program also provides for the joint employment of some visitors in consultative and research capacities with ministerial agencies of the government. The scholarship program by which Venezuelan students are sent to study at foreign universities will likewise be expanded. The committee will assist in the placement of the students in universities which offer outstanding curricula in their proposed fields of concentration.

James Silverberg from the University of Wisconsin will join the Department of Sociology and Cultural Anthropology of the Central University as Anthropologist in September 1953. Additional staff for the department, as well as for the Department of Economics are being recruited under the foregoing plan for the academic years 1954, 1955 and 1956.

University of Washington. Lincoln Armstrong has resigned to take a position at the American University in Beirut. Elizabeth Lyman, who has recently been engaged in graduate study at the University of Chicago, has been appointed for the academic year 1953-54 and will teach the courses in the same fields handled by Dr. Armstrong.

The following members of the department have accepted teaching positions: Ely Chertok at the University of California at Los Angeles; Charles D. McGlamery at Emory University; Frank Miles at Bowling Green University; and Judson B. Pearson at the University of Colorado.

* * *

News has reached the *Review* of the untimely death of J. Neely Boyer, an active member of the Society, in a plane crash in the Pacific Ocean in July, 1953. Dr. Boyer received his Ph.D. degree at the University of Pittsburgh in 1947 and was Professor and Chairman of the Department at Otterbein College, Westerville, Ohio.

The *Review* announces with sorrow the death of Lucius Moody Bristol, Professor Emeritus of Sociology, on May 9, 1953 at Gainesville, Florida. Professor Bristol earned the Doctor of Philosophy degree in Social Ethics at Harvard and taught at

Tufts College, at Brown University and at West Virginia University, then went to the University of Florida in 1920 where he was Head of the Department of Sociology and Economics until the founding of the School of Business Administration in 1926. In 1945, after 25 years at the University of Florida, he retired as Head of the Department of Sociology.

ERLE FISKE YOUNG

Erle F. Young, Professor of Sociology of the University of Southern California since 1924, died in Modesto, California on June 1, 1953. He had retired from active teaching in February.

Erle F. Young will be remembered by sociologists, faculty associates, and students as a great teacher and friend. They knew him as having a warm personality, an exceptional sense of humor, unswerving loyalty, and always seeing things in terms of a broad perspective. His students will remember him not only for these characteristics, but also for his ability to present clearly and forcefully the essential aspects of any problem. Moreover, he always made the problem of the student his own personal problem.

His broad scholarship is shown, in part, by the organizations to which he belonged. He was a fellow of the Academy of Political and Social Sci-

ences, and a member of the American Sociological Society, the American Statistical Association, the Rural Sociology Society, Pacific Sociological Society, American Association of Social Workers, California Conference of Social Work, Commonwealth Club of San Francisco, and Phi Beta Kappa. He was ever on the alert in seeking new problems for investigation. At the time of his death he was working on a two-volume book on the history of social work. He was author of *The Case Worker's Desk Manual*, *The New Social Worker's Dictionary*, and of many articles for various sociological journals.

The people of the community in which he lived, Modesto, California, held him in great esteem and have expressed their friendship and neighborliness through the establishment of an Erle Fiske Young Memorial Scholarship Fund at Modesto Junior College. The fund will be utilized for the awarding of scholarships to America's future farmers. Farming was an occupation which Dr. Young, from both a practical and scientific angle, rated as being of prime importance.

Dr. Young leaves his widow, Dr. Pauline Young; a daughter, two sisters, and five grandchildren.

HARVEY J. LOCKE

University of Southern California

BOOK REVIEWS



Engagement and Marriage. By E. W. BURGESS and PAUL WALLIN. Philadelphia: J. B. Lippincott, 1953, xii, 819 pp. \$5.50.

The long awaited report concerning the monumental Burgess-Wallin research with engaged couples appears as a book, big, important and good, but somewhat strange. The authors have chosen, perhaps in accordance with imperatives of the publishing business, to scatter their research findings through the pages of a book usable in a course dealing with preparation for marriage.

Following an introductory chapter relating social trends to family life, the authors describe their collection of 1,000 sets of schedules from the 6,000 sets distributed. A second set of schedules was obtained from each of the 226 couples actually interviewed prior to marriage. Couple members filled out schedules separately if interviewed and when studied at the later stage of marriage. Lack of collusion is assumed but not proven, however, for the general group of engaged couples. Checking on no-response-persons and the follow up of engaged couples was furthered by information from persons co-operating in the distribution of the schedules. Complete marriage schedules were obtained from 666 couples for valuable engagement-marriage comparisons. It is estimated that 150 couples broke their engagements. Of the couples previously interviewed as engaged, 124 were interviewed as married. The authors give a judicious account of their middle class group of subjects and inferences which might be drawn from knowledge of such subjects.

The findings to be presented to students, counselors and sociologists are set by the authors in a framework of concepts which take account of change, companionship marriage, decline of romantic love, emancipation of youth, waning of sex taboos, and the family life movement.

More specifically, the chapters on dating, going together, and love-idealization are organized with reference to Waller's theories concerning a period of dalliance and conflict-frustration as a source of love. Evidence is marshalled with some apparent effort leading to the conclusion that Waller's theory of dalliance is not in accord with facts concerning seriousness of early relationships. The Waller theory of idealization as based in part on sex frustration is rejected in favor of the authors' own view that

love can best be explained as identification with the loved person and that idealization occurs in proportion as needed to maintain self-esteem.

In the course of testing Waller theories, summarizing related research, and presenting extensive illustrations from case material, many interesting findings from the Burgess-Wallin research are brought forth. Especially interesting are findings concerning prior cross sex pairing, sex differences in physical intimacy, trend of love experience, prior engagements, discussion of contemplated marriage, doubts concerning engagement, emotional difficulties, and broken engagements.

While Waller's "principle of least interest" is not mentioned, certain evidence does bear on this valuable concept. In response to a question, "Do you ever grant demands or give way to your fiancé(e) for fear of weakening his (her) affection?", 37.5 per cent of the men and 22.3 per cent of the women responded "occasionally" or "frequently." Burgess-Wallin explain the greater apparent security of women by alleged compensation of women for lack of courtship initiative through pretense of being hard to get (p. 270). The reviewer would prefer a simpler explanation, namely that women subjects lied to Burgess and Wallin about their actual insecurity—and with safety since they did not contemplate marriage to these researchers.

The authors do have a splendid opportunity to check on premarital idealization because of their unique "before and after marriage" data. Subjects definitely rate the year after marriage as happier than the year before and do not clearly rate down physical appearance or personality traits of partners after marriage. The evidence does reveal, however, a tendency to desire more changes in the partner after marriage (p. 238). Also premarital sex intercourse is slightly associated with less approval of traits of the engagement partner (pp. 240-241).

A distinctive feature of the Burgess-Wallin research is the development and application of a scale for measuring engagement success. The schedule was patterned after the Burgess-Cottrell marriage adjustment scale. Male and female partner scores correlated to the extent of .57 and the test-retest reliability was .75 for men and .71 for women. The scores for unbroken engagements were significantly higher than for broken engagements in the case of both men and women. There was no very consistent rela-

tionship between duration of engagement and engagement success scores (p. 317).

Chapters 11 and 12 include valuable Burgess-Wallin evidence, together with evidence from Terman and others concerning the controversial issue of premarital sex intercourse. Incidence and correlates as revealed seem in accordance with expectations from other evidence. Engagement success scores are slightly higher for persons without premarital sex experience. The relationships of premarital sex experience to sexual adjustment in marriage and to marital adjustment are too complicated for summary in this review, but no clear evidence appeared that premarital sex behavior is the *cause* of later unfavorable sexual and marital consequences.

Following a chapter reporting opinions concerning marriage, comes a theoretical chapter on interpersonal relations in modern marriage in terms of intimacy and development. This chapter, like others, contains numerous brief illustrations from case material but few statistical facts.

The two following chapters, concerned respectively with measuring and predicting success in marriage, contain the research findings of greatest interest to family sociologists. Following a review of studies of marital success using single and composite criteria (e.g. the Burgess-Cottrell scale), the authors recommend the use of eight *multiple* criteria. They present schedules assumed to measure, respectively, evaluation of permanence, conceptions of marital happiness, general satisfaction with marriage, specific satisfaction, consensus, love, sexual satisfaction, companionship, and compatibility of personality. It is pointed out that permanence, happiness and general satisfaction are general scores. The last two correlate to the degree of .82. The specific scores correlate with general scores from .44 to .65 (p. 504). The authors' claims for validation of composite scores by divorce data and for the general applicability of scales for measuring marital success are not quite convincing to the reviewer (pp. 505-506).

With respect to prediction, the authors make a neat eight step analysis of prediction study procedure with comments on the final use of a trial group and the importance of other premarital background information. Items found predictive by Burgess-Wallin and others are grouped in the categories, parent-child interaction, social participation, economic behavior, sex attitudes and behavior, engagement history, personality factors and contingency factors. The criterion for listing items in summary tabulations is a C. R. of 2.0 or more and corroboration by material obtained during engagement (Adams, Terman-Olden, Burgess-Wallin). This criterion (p. 513) seems to be repeatedly

violated in the following tables in that corroborating studies are not mentioned (pp. 514, 516, 517, 518).

The sociologist seeking the essence of the Burgess-Wallin predictive achievement is annoyed with the vague statement, "They found a Pearsonian correlation of .31 for husbands and .27 for wives between background scores and marital success scores" (p. 519). Which scores and how derived? Again as to engagement history, the sociologist learns only, "Burgess and Wallin found a correlation between the engagement history items and a multiple marital success score of .28 for 600 husbands and .26 for 600 wives" (p. 523). How about the operations which give meaning to a correlation?

More detail is given concerning personality items, including test-retest correlations in terms of "T". Responses of partners are ingeniously combined and correlated with operationally defined engagement success scores. The correlation for men was .25 and for women .18 (p. 536). The rather startling conclusion is drawn from these correlations that a battery of such items could be used by engaged couples with a "reasonable degree of confidence" (p. 537).

By contingency factors are meant factors observable after marriage, such as duration of marriage, residence, economic factors, relations with in-laws, sex compatibility in marriage, and children. Burgess and Wallin studied "*anticipated*" contingency factors, presumably expectations reported on the engagement schedule. Their correlations were only .21 for 600 husbands and .19 for 600 wives (p. 547).

By this point the sociological reader has appetite whetted for the correlation of engagement success with marriage success. He is presented with a coefficient of .39 for men and .36 for women, but is given little clue as to what marriage success score is involved in this important finding (p. 548). The reader's confusion is not dissipated by a report of a new expectancy table relating prediction factors to "a multiple criteria" (p. 551). Here the criteria are named, but how they become a criterion is not indicated, and thus the reader is left to ponder upon a coefficient of .50. Some experimentation with new groupings of items and combinations of data from both husband and wife seems promising.

A chapter on forecasting marital success in the sense of making informal non-statistical guesses from general observation or by use of case material has some interesting features. Forecasts by parents have some slight significance as measured by the criterion of performance. The forecasts of close friends and of engaged persons are still more accurate with respect to future divorce or separation. A com-

bined forecast correlated .30 with the general marital satisfaction scores of 200 husbands in contrast to .36 for the engagement success score (p. 567).

Following a discussion of generic considerations in forecasting marriage success and marriage trends, a 14 point rating schema is presented which was used by 30 persons in predicting marital success of 200 couples from interview material with apparent improvement over unguided judgment. A combined forecasting score correlated to an almost embarrassing degree with an again unspecified "marriage adjustment score." The coefficients were .42 for the men and .39 for women as compared with .32 for the men and .38 for the women when the "engagement adjustment score" was used as a predictive device (p. 587). This reviewer is puzzled when the authors conclude, "These findings indicate that at present, case-study forecasts are not superior to statistical prediction and may be inferior" (p. 588). They do, however, see promise in case-study forecasting, especially with reference to trends in marriage.

Two chapters on marital adjustment and adaptability constitute an interlude in the presentation of Burgess-Wallin findings, although some interesting observations are made partly for the sociologist and partly for the student.

In chapters on the sex factor and children in relation to marital success, the authors release more of their findings. Their data seem to show a stronger sex drive in males as compared with females, and on the sexual satisfaction scale, which they adequately describe, women make significantly higher scores (p. 674). Some theorizing tends to the conclusion that sex adjustment is profoundly affected by other aspects of marital adjustment. The correlation of .49 between sex-adjustment scores and marital happiness scores is the same for husbands and wives (p. 690). Yet applying various scores measuring marital success, it is found that men seem to have marital adjustment dependent on sex adjustment, while women are more likely to have sex adjustment dependent on marital adjustment (p. 694).

Burgess-Wallin find with other researchers that desire for children, rather than having or not having them, is associated with marital success (p. 718).

In the final chapter there is a useful discussion of the implications of research findings and development for marriage with emphasis on the clinical value of upper and lower quartile scores. While there is recognition of pros and cons in applying measurement and prediction devices to family life education, the authors

are inclined to the pro point of view. An appendix lists some highly promising research projects pertinent to the various chapters of the book.

Such an important and outstanding book deserves some minor criticisms. (1) Some rather important findings, such as those of Ehrmann and Stroup are ignored. (2) The Burgess-Wallin research is hard to disentangle. (3) The critical ratios given in footnotes are not easily matched with tables. (4) The consistent use of bar diagrams does not always further easy interpretation. (5) Citations are not sufficiently complete and specific for the sociologist reader.

The most general comment would be that Burgess-Wallin have done the finest research yet achieved in the field but with a mode of presentation which leaves something to be desired by both college students and family sociologists.

CLIFFORD KIRKPATRICK

Indiana University

Probation and Social Adjustment. By JAY RUMNEY and JOSEPH P. MURPHY. New Brunswick, New Jersey: Rutgers University Press, 1952. xviii, 285 pp. \$4.50.

This book is the joint product of a Rutgers University sociologist (Rumney) and the long-time Chief Probation Officer of Essex County, New Jersey (Murphy). It gives a reasonably complete account of an extremely interesting effort to make a serious scientific follow-up study of probation as practiced by the Essex County, New Jersey, Probation Office. In many ways it is perhaps the most ambitious and far-reaching study of probation ever attempted any place in the United States. If the results are something less than definitive in every respect, they are nevertheless of very real importance for everyone concerned with correction and the basic problems of the administration of justice. It is a "must" for everyone interested in the field.

More specifically, the study is concerned with the first 1000 persons placed on probation in Essex County in 1937. The object of the research was to determine the "social adjustment" of this group eleven years afterwards, in 1948, and to evaluate the probable effectiveness of their 1937 probation experience in relation to their "adjustment" in 1948. For the implementation of this involved project somewhat less than adequate procedures were applied that gave little critical attention to the central problem of the significance and reliability of the basic information collected.

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the first 1000 consecutive cases admitted to probation after January 1, 1937 (by June 1937 the desired number had been obtained)—the cases had to be located in 1948 in order to interview them "according to a questionnaire schedule drawn up beforehand and designed to yield the information required for evaluating the subjects adjustment" (p. 7). Of the original 1000, 111 had died by 1948 and only 672 were actually interviewed, that is, 67 per cent of all cases and 75 per cent of those living in 1948. Forty-two cases were located but refused to be interviewed, and another 68 were located but effective interview contact was never accomplished.

The age range of the sample extended from 10 years to 73 years, and reflects the fact that cases came to the probation department from all of the different courts in the county. Exact figures are as follows: 333 Criminal Court cases, median age 34; 309 Domestic Relations Court cases, median age 34; 227 Police Court cases, median age 28; and 131 Juvenile Court cases, median age 14.

Basic research information came from two principal sources, namely, the official probation record and the follow-up interview. The official record centered on the 1937 offense and probation experience, but involved also supplementary information about prior to 1937 criminal record and subsequent contacts with law enforcement agencies and with other social agencies. About one-third had a pre-sentence investigation report in 1937. The follow-up interview was "designed to discover not only facts on present circumstances and conditions, but also reactions to the probation experience, utilization of leisure time, and general philosophy of life" (p. 7). As to the reliability of all this, the authors say, "In only a few cases was it possible to re-interview the probationer and thus check for accuracy of replies. In the main, we believe the information is reliable in so far as we could check with original material and with the prior and subsequent court histories of probationers and their social service contacts" (p. 11).

Setting out to view their cases as "people in trouble" rather than as "criminals," and with the general notion of determining the effectiveness of probation treatment from the "improvement" shown in their cases in the eleven years between the 1937 probation experience and the 1948 follow-up, a large part of the book is given over to the elaboration of procedures and methods devised for dealing with this problem. "Adjustment" was determined objectively, i.e., according to a carefully elaborated schedule of factual information, in four general areas of problems, namely, "physical," "mental," "fam-

ily," and "economic." Comparative figures for the two periods, in terms of the per cent found to be "adjusted" are given as follows:

Problem Area	Per Cent Adjusted	
	1937	1948
Physical	87	87
Mental	83	85
Family	51	77
Economic	65	81

The authors point to the "improvement" shown in the area of family and of economic problems, but admit that the economic improvement may have been due as much or more to the "prosperity" of defense and war production that followed 1937 as to any positive influence from the probation experience. They also admit that the "improvement" in the area of family problems may have been as much a function of the improved economic condition of the cases as to any help or guidance deriving from the 1937 probation experience.

Interestingly enough, the prior and subsequent criminal records are remarkably similar. In terms of the degree of seriousness of criminality involved on the part of the 889 living in 1948, the results are as indicated in the simple table below: (p. 160)

Kind of Offense	Between 1937 and 1948		Prior to 1937	
	No.	Per Cent	No.	Per Cent
None	430	48	419	47
Minor only	231	26	282	32
Major only	77	9	57	6
Both major and minor	151	17	131	15
Total	889	100	889	100

Space does not permit any further listing of details. Full analysis is made of age differences, type of court, length of time on probation, manner of discharge, and so on.

There is a final chapter on "Improving Probation" that seeks to bring the study to a focus by spelling out implications and giving systematic suggestions for sensible next steps in further development of probation services. This is non-controversial and few, if any, will disagree with the pattern of improvement suggested.

The research attempted made no effort to deal with the selective factors involved in being placed on probation—that may be much more important in determining outcome than the service provided by the probation department. Similarly, there was no attempt to make use of a "control group" of those who did not have the benefits of probation experience with which to compare the results found for those who did experience pro-

bation control. Much more definitive kind of research on probation still needs to be undertaken.

GEORGE B. VOLD

University of Minnesota

Out of Step: A Study of Young Delinquent Soldiers in Wartime; Their Offences, Their Background and Their Treatment under an Army Experiment. By JOSEPH TRENAMAN. New York: Philosophical Library, 1952. xx, 223 pp. \$4.75.

This piece of research has several interesting facets. It concerns the treatment and outcome of approximately 200 young Army offenders who were sent to one of several Special Training Units of the British Army during the war period. The treatment situation was certainly very much more open, permissive, and informal than an ordinary disciplinary barracks. The author was one of the education specialists at this retraining center. The subjects were under 21 years of age and it seems that normal types of Army punishment and discipline had done them no good whatsoever. The approach was primarily one of social re-education in a nonpunitive, relaxed setting. Only about 4 per cent of the new arrivals "ran" from the program.

The author makes great use of questions which attempt to measure how the lads responded to the program and how they felt about the program. It seems that the lads, in the majority of instances, received very positive value from the retraining—in their own estimation.

The overwhelming majority of the trainees were restored to Army duty and the results in terms of personal adjustment from here on until the post-discharge period were quite favorable. Assessments of the lads' re-adjustment to the Army by regular Army staff members were skewed toward above average ratings, compared with similar staff evaluations of regular Army privates. More than two-thirds of the trainees received a normal discharge whereas less than one-fourth received an unsatisfactory rating at the time of discharge.

One out of three in the sample had appeared before civilian courts before entering the Army. The other two-thirds of the sample might well be said to have emerged from the fringe of potential delinquency.

When compared with a control group of non-trainees from the British Army, the trainees showed up as a much more disadvantaged group, in areas of health, education, intelligence, neighborhood conditions, and family situation.

The author, from information collected by interviews, life histories, and schedules, was able to develop a count on various factors present in

the background of each case history. These factors primarily were concerned with social background conditions, although there were some psychological traits recorded by tests and clinical examinations.

It is interesting to note that the author finds an average of 3.6 factors per case which, you will remember, is just about what Dr. William Healy found in his pioneer work, *The Individual Delinquent* (1914). In the factors that were considered major factors, about 80 per cent of such factors had direct bearing on the family situation. Psychopathic personality showed up in about 19 per cent of the cases, practically all of which were discharged as total failures, but interestingly enough the social background conditions of psychopaths did not seem to differ from the social background conditions of the rest of the group.

In this modern age when psychoanalysts and psychiatrists hold the Western World spellbound with their pronouncements about the depth-level dynamics of behavior, it is refreshing to hear once again from a study which reaffirms the importance of the social situation in determining delinquent behavior. The now-famous Professor Sir Cyril Burt, in his preface, thinks very highly of this piece of research. The writer shares his considered opinion.

WALTER C. RECKLESS

Ohio State University

Crime in Modern Society. By MABEL A. ELLIOTT. New York: Harper and Brothers, 1952. xvi, 874 pp. \$6.00.

This new textbook in criminology has several features that should make it quite acceptable to a variety of groups. It is not only intended for college students but it should also appeal to various professional groups engaged in work with criminals. The lay reader interested in modern crime problems can also read and study it with profit.

The general organization of the book follows what Miss Elliott conceives modern criminology to be. To her "criminology may be defined as the scientific study of crime and its treatment. To date this study has produced a large body of facts which relate to (1) the nature of crime, (2) factors which are associated with criminal behavior, (3) case studies of offenders, and (4) the treatment or punishment of criminals" (p. 24). In accordance with these endeavors of criminology the book brings together selected portions of the facts and opinions found or stated by some of the leading criminologists of the past and present into seven sections. These

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sections deal with: the nature and extent of crime; criminals; factors in criminality; the treatment of criminals; crime prevention. Thus, the book centers on topics that have come to be regarded as constituting the essential phases of modern criminology. The reader will find that the materials in each of the sections are well organized and presented in an interesting fashion. The portions devoted to the female offender are especially complete and unique. Most other authors of textbooks in criminology tend to give this phase of the crime problem scant attention.

The section on factors in criminality will, it is believed, be received by many critical readers with mixed feelings. Its rather polemic character and the eclectic treatment of the materials in this field will tend to detract from the presentation for some readers. To others this very character of the discussion will be most attractive. No one will, however, seriously quarrel with Miss Elliott when, after reviewing a good portion of the work done in the etiology of criminal behavior, she concludes that "any objective consideration of theories of criminality indicates that the various hypotheses need further testing. For example, there must be more study of the offenders' characteristics in contrast to those of non-offenders (if any), and the precipitating effects of environmental influences must be studied for whatever insights they offer into why the criminal behaves in an anti-social fashion" (p. 404).

Miss Elliott's book is one of the better of recent textbooks in criminology. It contains recent research findings and should give the reader a rather thorough introduction to the whole field of criminology.

ELIO D. MONACHESI

University of Minnesota

Social Problems and the Changing Society.

By MARTIN H. NEUMEYER. New York: D. Van Nostrand Company, Inc., 1953. ix, 477 pp. \$4.25.

This reviewer was first called upon to teach an undergraduate course in social problems in 1947. At the time there were only two texts which could be considered to be even nearly suitable, and both were revisions of books first published in the nineteen thirties. The evidence from a newer, if more cursory, survey indicates that there are now several recently-published works in this field superior to anything available at the earlier date. Professor Neumeyer's book is a worthy addition to this group.

The first four chapters are a broad description of the conditions of change in modern society, together with an analysis of the approaches utilized by sociologists in the study of social disorganization. The next ten chapters are devoted to description of specific problems. These include most of the problems usually discussed in texts of this sort, with the unusual inclusion of a chapter on housing. The last two chapters are devoted to what may be considered to be "solutions" from the point of view of social control and planning.

The general impression gained from the reading of this text is that it would be highly teachable. That is, the organization is coherent and the style is such that the book could be understood by the freshman and sophomore students for whom it is primarily designed. The citations and references are reasonably complete and, where appropriate, recent. There are several minor points in interpretation of data to which one reader or another might take exception, but it is difficult to imagine a text in this area so innocuous that such differences would not occur.

Any attempt to evaluate this text as a contribution to a systematic theory of social disorganization would be unfair, for the making of such a contribution does not appear to be its primary purpose. The book is obviously not a carelessly-assembled collection of classroom notes—it is the product of considerable thought and work, but it is not startlingly original in approach or in analysis. The more sophisticated might consider parts of the presentation to be superficial—merely the stringing together of descriptions of conditions with which all are, presumably, more or less familiar, and without cogent analysis of underlying causes—although Professor Neumeyer does demonstrate a knowledge of the complexity of social causation. The teacher, harassed by the problem of finding a workable, interestingly-written text, may be less critical.

At a number of points reference is made to the fact that values and value systems are one of the determinants of what are considered to be social problems. The general orientation of the book—Professor Neumeyer's unstated system of values—is a part of a politically-liberal, humanitarian tradition which stems, in American sociology, from Ward. This reviewer can see no objection to some indoctrination of students in this point of view.

DONALD D. STEWART

University of Arkansas

Asian Nationalism and the West. Edited by WILLIAM L. HOLLAND. New York: Macmillan Co., 1953. viii, 449 pp. \$5.00.

A rich field for sociological analysis, especially in the fields of political sociology and collective behavior, is provided by the rapid political developments taking place in Asia. The complex variety—and often rapid succession—of nationalistic movements, the collective struggles to throw off colonial domination, shifts in the relative strengths of new political parties in “new nations”—these and many related collective phenomena offer the sociologist an absorbing set of group experiments for comparative study. Parts of the present volume contain useful source materials for such a purpose.

The book is promoted by the publisher as “an international symposium on nationalistic movements in Southern and Eastern Asia.” Edited by the Secretary General of a social movement, the Institute of Pacific Relations, it is actually a collection in book form of (1) three revised “data papers” originally prepared for the Eleventh International Conference of the Institute held in Lucknow, India, in October, 1950, (2) the summaries (66 pages) of the five round-table discussions held at the Conference, (3) a short address before the Conference by Nehru, and (4) an introduction to the book by the editor.

The three data papers, which make up about two-thirds of the book, deal with nationalism and politics in selected Asian areas from shortly before World War II until 1952. The first, “Indonesian Politics and Nationalism,” is by George McT. Kahin, Director of the Southeast Asia Program, Cornell University; the second, “Vietnamese Nationalism and French Policies,” by Philippe Devillers, Professor, Collège Libre des Sciences Sociales et Economiques, Paris; the third, “Nationalism in Malaya,” by T. H. Silcock and Ungku Abdul Aziz, both of the University of Malaya. These writers are, respectively, a political scientist, a historian, and two economists. While many statements throughout the book emphasize the obvious fact that Asian nationalism has followed unique paths of development in the different countries, the present reviewer was impressed with the contrasts between the types of materials selected for analysis in the three papers although all were dealing with the same general subject. These contrasts, in considerable degree, appear to reflect differing disciplinary frames of reference used by the respective analysts. Placed one after another in the book, the apparent unevenness and varying emphases of the three papers

underline once more the urgency of a more integrated social science approach. In spite of these differences, however, the student of collective behavior will find interesting, scholarly accounts of a large number of political movements, particularly in Indonesia and Malaya, which reflect a wide variety of positions on a right-left ideological continuum. The interrelation between these movements and religious, labor, youth, and totalitarian movements is also touched upon.

The sixty-page introduction includes materials from other Conference data papers concerned with the nature and effects of nationalism in India, Ceylon, Burma, the Philippines, China, and Japan. Some of these materials, together with Professor Kahin's paper on Indonesia, throw important light upon the changes in ideology and tactics which take place as a nationalist movement bent upon opposition during the “liberation” or anti-colonial phase must suddenly assume the sobering role of operating the institutions of government and accept appalling responsibilities for the economic, political, and social reconstruction of “under-developed” countries in the face of both internal and external opposition.

Primarily due to its short-time focus—a period of roughly fifteen years—the book will soon become dated for the general reader, although for the time being it will provide him with a valuable perspective for interpreting many current developments in the Asian world. For several sociologists it will have more enduring usefulness as a casebook for American students of political sociology and social movements who need increasingly to approach these fields from a global point of view.

CLARENCE E. GLICK

University of Hawaii

Readings in Latin American Social Organization and Institutions. By OLEN E. LEONARD and CHARLES P. LOOMIS. East Lansing: Michigan State College Press, 1953. viii, 320 pp. \$5.00.

Publication of this volume makes available a collection of widely scattered essays about Latin America, thus facilitating the task of “social science” teachers and students interested in studying that area. It is to be hoped that it will also stimulate further research in their field. Two of the thirty-six articles, which have been grouped into ten “Parts”, were written by historians while the remainder, with the exception of one written by a lawyer, are rather evenly divided between sociologists

and anthropologists. A suitable sub-title would have been "Latin America through the Eyes of American Social Scientists," for all but two of the authors have been permanent residents of the United States all their lives, and skilled and learned as all of them are, there is little indication in their writing as to how the people of Latin America themselves look at their world. One of the two "non-Americans" is a "Latin American" who resided many years in Europe and the other is a European who lived in Latin America for many years, but is now a resident in the United States.

With six exceptions the papers have appeared in standard social science journals with the *American Journal of Sociology*, the *American Sociological Review*, the *American Anthropologist* and *Social Forces* carrying off the honors. The two final chapters or "Parts" dealing with bibliography also largely make reference to items in the same or similar reviews. Some very good material may have been missed by the editors in perpetuating this inbreeding tendency in "American" social science in a volume which they hope will be used "to acquaint students with Latin America as a cultural area" and not simply serve as a text to teach social science methods.

The reviewer does not wish to enter into a controversy with the editors as to whether they made the best possible selection or not, especially since it would not be possible to do so within the limitations set for this review. It was surprising, however, not to find a discussion of the sociological aspects of inter-American relations, an exposition of some of the social aspects of economic development, or a description and analysis of the growth of a state as a social institution. Even "American" political scientists and economists have been writing articles about Latin America which should be of great interest and value to sociologists, yet there is hardly even an indication of awareness of such material. Discussions of two other very significant institutions—local government and trading centers or markets, have also been omitted. Since both of the editors have spent considerable time studying the Spanish-speaking population of the southwestern United States, one might have expected them to include at least one article about Latin American social organization and institutions in the United States, but this subject is only mentioned in the bibliography. Excellent as the papers are, they fail to present a balanced picture of Latin America. They hardly do more than mention, for example, the life of the dominant classes or of the large urban and industrial centers.

If the United States were to be similarly described even the most objective sociologist or student of social problems would be likely to object violently. This is much more an indication of the limitations of the subject matter of the articles available, however, than it is of the point of view of the editors.

As "social science" has matured, increasing attention has been given by social scientists to analysis of their own work. The reviewer wishes it had been possible for the editors to prepare a lengthier preface in which they discussed, in general terms, the work which has been done so far by social scientists in the Latin American field, and what gaps in knowledge most urgently need to be filled.

The authors have made an important contribution to the availability of good material for social science courses on Latin America but as has been suggested, that need has hardly begun to be met. In addition to the comments which have already been made, it might be mentioned that during the last few months a number of very excellent articles on the same and closely related subjects as are included in this volume have been published which might perhaps have been included if they had appeared earlier. Students in the United States should also have an opportunity to become acquainted with the work of Latin American scholars and to know how the Latin Americans look at their society and institutions.

ROBERT CUBA JONES

United Nations

Bolivia: Land, People, and Institutions. By OLEN E. LEONARD. Washington, D. C.: The Scarecrow Press, 1952. 297 pp. \$6.00.

This is one of the few books published by sociologists in the year 1952 that is certain to have a lasting value. Theoretically adequate and methodologically sound, it has been done in a frame of reference that permitted the analysis to be both comprehensive and detailed without being at the same time abstruse, pedantic, and boringly repetitious. All in all it represents American sociology at its best at grips with the real problem of observing, analyzing, describing and interpreting a highly significant society and civilization. It may be that few of Leonard's fellow sociologists will pay any particular attention to the book he has produced, but the same certainly is not true of those in other scholarly fields and those who for any reason are seeking significant, tested, organized information about Bolivia and other parts of Latin America. Along with somewhat similar efforts by Carl

C. Taylor, Lowry Nelson, Arthur Raper, Irwin T. Sanders, Nathan L. Whetten, and an encouragingly increasing number of others, *Bolivia: Land, People, and Institutions* is a basic contribution to a sound knowledge of hitherto sociologically unknown parts of the world. It will help gain for sociology a greater degree of respect in the world of learning.

Features of the book include a lengthy demographic analysis of the population of Bolivia; a thoroughgoing analysis of land tenure and other basic institutional relationships of man to the land; comprehensive treatments of the fundamental domestic, educational, religious, and governmental institutions; and a highly informing study of levels and standards of living, including chapters devoted to housing, the Indian stereotype, clothing and diet, health and sanitation, and recreation and amusement. Several maps, some well-designed charts, an excellent selection of photographs, the judicious use of tables, a glossary of terms, and a comprehensive bibliography add greatly to the effectiveness of the presentation and the scholarly value of the book.

Leonard took to his task some ten years of sociological research experience in the United States, and in Central America, Ecuador, and other parts of Latin America. He had a thorough knowledge of Spanish, including a fluency in speaking that tongue. He spent two full years in Bolivia visiting all parts of that highly diverse country, carrying on detailed socioeconomic studies in selected areas, and helping develop and guide scientific efforts to improve agriculture and rural life throughout the nation. The net result is highly gratifying. The book he has produced will be the standard work on Bolivia for many years to come.

T. LYNN SMITH

University of Florida

Racial and Cultural Minorities: An Analysis of Prejudice and Discrimination. By GEORGE EATON SIMPSON and J. MILTON YINGER. New York: Harper and Brothers, 1953. x, 773 pp. \$6.00.

With a variety of textbooks and a large number of research studies dealing with minority group relations available, it is not easy to make a distinct contribution to the field. Yet, the present volume succeeds in making such a contribution. It does it by cogently analyzing and summarizing the information made available by various studies, historical, psychological, anthropological, and sociological so that the reader gets

a clear picture of the intricate problems involved in minority-majority group relations.

The book is divided into three parts, the first dealing largely with the causes as well as the consequences of prejudice and discrimination, the second—with the status of minority groups in the social and economic structure of American society, and the third—with the proposed means of reducing the incidence of prejudice and discrimination.

Nearly half of the volume is devoted to a discussion of the causes and the various aspects of prejudice. A rather thorough and many-sided analysis of anti-Semitism, the classic case of prejudice, is used as an illustration of how prejudice works and of the effects it has on both the victim and the offender.

In the discussion of the status of minorities in American life and the problems they present, the economic, political, legal, familial, religious, educational, and artistic aspects are considered. The groups singled out for most intensive analysis are the largest minority, the Negroes, and the most distinct white minority, the Jews. Other groups, however, notably the Mexicans, Chinese, Japanese, and Italians, are frequently brought in and comparisons drawn. Some sixty-odd pages at the end of the volume are devoted to a consideration of the techniques and devices advocated for dealing effectively with the phenomenon of prejudice and discrimination.

This reviewer finds himself in complete agreement with the authors as regards the approach, manner of treatment of the subject, the allocation of space to the different topics usually included in a book of this nature, and the emphasis given to them. The decision to dwell at length on the problem of prejudice and its consequences and to emphasize just a few minorities instead of attempting to cover most of them for the sake of "completeness" made it possible for the authors to analyze the problems more thoroughly and meaningfully. After all, in analyzing the case of one or two minority groups, if done properly, a great deal can be learned about the rest, as all ethnic minorities share very many problems in common. Similarly, the stress laid on the role that personality factors play in prejudice adds greatly to the reader's understanding of its origin and nature.

The authors have spared no effort to draw upon the extensive literature in the field and to utilize its findings and conclusions. The various theories relating to the problems of minority groups discussed are examined and evaluated objectively. Moreover, the attempt is made to consider the problems in relation to the general principles of human behavior rather than by themselves. The result is that the reader is af-

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forded a good insight into their complexity and ramifications.

It is the conviction of the present reviewer that this book is bound to become a standard text in the field and to prove a boon to students and teachers of courses dealing with group prejudice and racial and cultural minorities.

SAMUEL KOENIG

Brooklyn College

Race and Class in Rural Brazil. Edited by CHARLES WAGLEY. Paris: UNESCO (Columbia University Press, distributor), 1952. 160 pp. \$1.25.

In 1950 Unesco's Department of Social Sciences initiated "a programme for the study of race relations in Brazil." The present careful, factual report was prepared by four persons, Charles Wagley, Harry W. Hutchinson, Ben Zimmerman, and Marvin Harris, who, prior to their agreement with Unesco, were already carrying on research in the four rural communities that are dealt with in this volume. The communities are regarded as representative of four different regions of rural north Brazil—a sugar plantation area, a former mining area in a mountainous region, an arid semi-desert region, and the Amazon Valley. Since over 70 per cent of Brazilians live in small towns of less than 5,000 people, or in the countryside, the four communities studied were considered representative of an important segment of the whole country.

The authors observe that the pattern of class and race relations in these rural areas, which are on the whole pre-industrial and pre-urban, is more typically nineteenth century and perhaps more "traditionally Brazilian" than will be found in the large cities of Brazil. Although each of the four communities showed local variations in status patterns, attitudes toward racial characteristics, and emphasis upon socio-economic attainment, comparison revealed a generally similar pattern of race and class relations. The authors see this pattern as an emergent from historical processes in which Brazilian society has been shifting from a caste basis to a system of social classes which are themselves still undergoing change.

The general features of this pattern are outlined. As a consequence of the persistence of unequal economic opportunities and educational advantages, upper class persons in the areas studied are still "almost exclusively Caucasian in appearance, and the majority of the 'people of colour' are found in the middle and lower classes" (p. 145). Nevertheless, numerous people of mixed ancestry are found in local upper class groups where the criteria for belonging are "one's

income, the kind of work one does, one's education, one's family (in varying degrees), and finally, one's physical appearance" (pp. 148-149). It is in the competition for membership in the local upper class and for social ranking within it that "the criterion of race becomes most crucial in determining social position" (p. 149). The absence of an absolute racial criterion for class status is indicated by the fact that the lower class groups contain people of all racial types. Moreover, while racial type is a criterion for relative social rank within classes, in "actual personal relationships . . . racial characteristics seem to be of little importance, being overshadowed by income, occupation, and other criteria both personal and social" (p. 149). Although the authors conclude that "a mild form of racial prejudice exists on all levels of society in rural Brazil" (p. 149), "this prejudice is mainly one of social class rather than one directly focused upon people having a particular set of racial characteristics" (p. 150).

The authors state that their studies generally reaffirm the hypotheses set forth in Donald Pierson's *Negroes in Brazil* (1942), based primarily on the study of the city of Salvador in Bahia. These include the hypotheses that Brazil is a multiclass society differing from caste societies and from those in which racial minorities are not accepted by dominant majorities, that prejudice is based on class rather than race, and that racial distinctions are not absolute barriers to improvement in social status in the total community. At the same time, they outline a number of supplementary hypotheses for further research on race relations in Brazil.

CLARENCE E. GLICK

University of Hawaii

Ethnic Relations in the United States. By EDWARD C. McDONAGH AND EUGENE S. RICHARDS. New York: Appleton-Century-Crofts, Inc., 1953. xiv, 408 pp. \$4.00.

The purpose of this book is "to present a factual picture of the statuses of selected ethnic groups in the United States in terms of a definite frame of reference." The three parts of the book deal with "understanding," "analyzing" and "improving" ethnic relations. In most of the fifteen chapters there are one or more readings from the work of specialists, twenty-three readings in all. This is not, however, a reader in race relations.

Following the argument of Ashley Montagu and others, the authors employ the term "ethnic group" rather than "race." Seven ethnic groups—Negroes, Jews, Mexicans, Indians, Japanese, Chinese and European immigrants—are examined in terms of four selected aspects of

status: social (interpersonal), legal, educational and economic. The old-line American white Protestant stock, the group which passes judgment on the seven and assigns status positions to each, is not, presumably, an ethnic group.

The nerve of the race problem, it has been said, is the struggle for status between groups identified as racial who are at the same time involved in an elementary struggle for existence. In the United States every citizen of whatever race or ethnic group has legally the same status but does not possess it actually. It is this fact that gives point to the remark, "All men are created equal but some are more equal than others." It is this fact, too, that makes it necessary for men to struggle and agitate to achieve the legal status to which they are entitled. During the struggle men discover what seem to be equalizers such as money, public office and education. It appears from the discussion in this book that the struggle for social status takes a somewhat different pattern in different ethnic groups.

On the whole the book is balanced and objective. The materials are clearly and simply stated. The result is a volume worth examination by those who employ a text in introductory courses in ethnic or race relations.

EDGAR T. THOMPSON

Duke University

The Refugee Intellectual: The Americanization of the Immigrants of 1933-41. By DONALD PETERSON KENT. New York: Columbia University Press, 1953. xx, 317 pp. \$5.00.

"Assimilation," Kent writes, "is a process of absorption. It is a process by which one body ingests a foreign body and so incorporates it that its original identity is lost and it becomes an indistinguishable part of the absorbing body." This total, if not totalitarian, conception of assimilation, borrowed from H. P. Fairchild, dominates Kent's study of the adjustments to American life made by a group of professional refugees who came to the U. S. from Germany and Austria during the 1933 to 1941 period. It is difficult to reconcile this concept of assimilation, derived essentially from a biological analogue, with the more recent sociological analyses of contemporary America in terms of cultural pluralism and the multi-group nature of our society.

This crucial conceptual error is only one of several serious limitations of this study. Somewhat sloppy in method, superficial in analysis, weak in conceptualization, and, at points, obtuse in its writing, Kent's book adds practically nothing to our previously acquired

knowledge of the adjustment of the Hitler-made refugee in America. M. R. Davie's excellent study of this subject is scarcely improved upon or added to, except that interesting anecdotal materials, derived from personal interviews, are presented.

With respect to method, serious questions can be raised regarding both the sampling design, including the inadequate treatment of the problem of nonresponse, and, judging from the incidental examples scattered throughout the volume, the quality of the interviewing. Although Kent recognizes the bias in the original sample selection (only cases with an agency contact are included), he minimizes this bias by stating that "it would seem logical to suppose that any error resulting from the present selection would be one unfavorable to the refugee, since those not applying for aid presumably either had means or relatively few problems." Why the use of logical supposition in lieu of empirical data? Does not "unfavorable" suggest an implicit value judgment which is out of place in a study which boasts of its scientific objectivity?

On the conceptual side, exception must be taken not only to the severe, limited concept of assimilation employed, but also to the author's cavalier equating of "professional," in the occupational sense, with "intellectual." Not all members of professions occupy the social status or assume the social role of intellectual. Moreover, Kent juxtaposes in a single paragraph ideas and quotations from H. P. Fairchild and M. R. Davie without apparently being aware of the fundamental conceptual differences in the writings of these two students of American immigration.

On the score of analysis, the author's superficiality is especially revealed in the chapter on learning the language. The treatment here is in terms of generally vague, common-sense notions without reference to the excellent experimental literature developed in recent years by learning psychologists. Kent has evidently combed quite well the materials in the assimilation field, narrowly conceived, but he does not seem to have perceived the relevance to his problem of the pertinent general sociological literature on mobility, the stranger, the social role of the intellectual, and the like.

Some highly questionable propositions are offered as "logical" (p. 41) or as "truisms" (p. 37) or as eternal truths (p. 244). For example: "The effects of American culture upon the refugee will, of course, be greater than the effects of the refugee upon American society. *This has ever been the case with immigrants*" (italics mine). Indeed! As, for example, the effect of American Indian culture

upon the first English immigrants to the U. S.? To ignore so completely all that anthropology and sociology teaches on the role of relative statuses and relative cultural levels in group contacts is inexcusable. Kent does, at one point, quote from one of Linton's acculturation studies, but apparently has missed the point.

The appendices contain all the trimmings of the proper research report: statistical tables, study forms, methodological note, illustrative letters from refugees describing their work experiences and adjusted estimates of refugee integration. This is very useful and helps the reader to evaluate the research materials himself. And the Columbia University Press is to be commended for an attractive printing job. But the basic question remains: aside from earning the author his doctoral degree, was it worth all the trouble?

HARRY ALPERT

National Science Foundation

Dynamics of Social Action. By SEBA ELDRIDGE. Washington, D. C.: Public Affairs Press, 1952. vii, 119 pp. \$2.50.

This volume may be described as a handbook for reformers. Seba Eldridge, the author, begins it with a brief resumé of the history of social change in America. He includes also a description of the nature of the process, emphasizing the importance of the time element. Social life, he indicates, has great inertia; only after prolonged efforts on their behalf can innovations succeed.

But even after the passage of much time and the expenditure of much effort, society still has "problems." Dr. Eldridge mentions a number of them as deserving serious attention from the citizens. The chief ones, presumably in the order of their immediacy, are: conservation of natural resources, utilizing talent and genius; developing appropriate interests in people, the problem of citizenship; the place of leisure-time; the organization of personnel; and the problem of competition.

The main part of the book is devoted to suggestions and directions, many quite specific, as to how citizens who are interested in the welfare of their communities can most effectively work for the desired ends. Attention is called, for example, to the corps of professional social workers already present in the community, whose aid may be enlisted on behalf of any action contributing to the general betterment. There are also the mass communication agencies, such as the newspapers, periodicals, radio and the movies, which serve in the first instance as sources of information upon which social action may be based and, secondly, may be controlled for

propaganda purposes to convince or persuade the public to accept or reject any contemplated action. The book further indicates how the political processes, the numerous groups in society finally, how even so-called pressure groups may be made to further the cause of improvement in the community.

The author's strong faith in the ultimate triumph of democracy underlies every sentence in the book, a view which, as he presents it, does not seem at odds with his conclusion that the individual, working alone, is helpless in the complicated structure of modern society. If there is any single bit of advice in the book which seems more important than any other it is this conclusion. No one need think that he can accomplish anything by himself. Social action is always group action. To bring about social change of any kind, the reformer must ally himself with organized groups, because only organized groups have the power required to introduce change into the present-day institutional system.

Throughout the book the author assumes the role of one advising those who are moved by a great good will to attempt the creation of a "better" society, meaning, obviously, a society as free as may be from the traditional ills—poverty, strife, crime, personal frustration, and other "bad" conditions. It may be fervently hoped therefore that only members of the intended reader groups will see the book. For the advice given therein may be followed with equal profit by those who would injure or destroy society for their private gain.

CARL M. ROSENQUIST

The University of Texas

Women in the Modern World: Their Education and Their Dilemmas. By MIRRA KOMAROVSKY. Boston: Little, Brown and Company, 1953. xv, 319 pp. \$4.00.

This book is a major contribution to the sociology of education; it is also a valuable contribution to the sociology of the modern family. For in the process of evaluating the education of women, Komarovsky casts a long and searching look at the role assigned to them today. To be more accurate, at the confusion in role assignment of women today. The author seeks to discover "what in contemporary society causes such widespread uneasiness in women and tensions in the relations between the sexes" (viii-ix). She rejects the psychological, psychoanalytic, and biological explanations and substitutes instead a sociological one in terms of inconsistencies in role patterns. The Industrial Revolution disturbed an old equilibrium and a new one has not yet been established. Old patterns persist which do not

mesh in with modern realities. "New conditions have arisen which have not as yet been defined by public opinion—leaving human beings without guidance and protection. New goals have emerged without the social machinery required for their attainment. The old and the new moralities exist side by side dividing the heart against itself" (p. 48). Komarovsky, by means of carefully analyzed and interpreted case data, documents her thesis for all stages in the life of middle-class, especially college trained, women. In the end she comes up with recommendations for the kind of curriculum colleges should offer to women in order best to fit their needs. Not uninformed skill in techniques, but the basic theory back of the techniques, perspective, a grasp of the significance of the techniques in a larger framework—these are the most useful contributions the liberal arts can make to the prospective homemaker. And since men are to become parents, too, their education should also be planned to prepare them for this role.

In brief compass, with beautiful economy, Komarovsky has presented a truly sociological diagnosis of the malaise of modern—that is, of college educated middleclass—women in the confused role situation of today, and proposed a way of dealing with it. In pursuing her argument she does not fall into the fallacies of the old line feminists; she clearly recognizes the existence of psychological sex differences which mass comparisons of the sexes reveal. But she is also concerned about the overlappings, about the girls who surpass boys in physics, for example—especially when millions of human beings are involved.

This brief sketch of the skeleton of Komarovsky's book gives no idea of the wealth of specific insights it contains. It is in the tradition of Cooley's work, lucidly and appealingly written. So much so, in fact, that one might easily overlook the depth of the insights it offers. It will certainly appear on the required reading lists of family courses in women's colleges and on the recommended lists of family courses in all other colleges. It is especially important for the men who have to live with modern women.

JESSIE BERNARD

The Pennsylvania State College

Future Citizens All. By GORDON W. BLACKWELL and RAYMOND F. GOULD. Chicago: American Public Welfare Association, 1952. xxix, 181 pp. \$2.00.

This is a nationwide study of the Aid to Dependent Children, a program which currently

serves one and one-half million children. The study was conducted by the Institute for Research in Social Science of the University of North Carolina for the American Public Welfare Association. It is based upon a sample of some 6,500 families (including 19,000 children) for whom Aid to Dependent Children payments were terminated in the fall of 1950 or early in 1951. The sample was secured with the co-operation of the public welfare departments of 38 states, the District of Columbia, and the Territory of Alaska.

The study contains a wealth of information concerning the crisis which had occasioned the application for aid, duration of aid, size and composition of the family, housing conditions, income, educational progress of the children, other indicators of children's welfare, and adjustment of the older children at the time of separation of family from A.D.C. National averages are broken up by rural and urban areas and white and non-white cases.

In comparing this study with previous studies of the Aid to Dependent Children the authors point out certain distinctive features of their research. Among these was the focus upon the effects of the program upon the children and the attempt to go beyond description and seek out significant statistical relationships between factors in the family situation and in child welfare.

The first of the overall conclusions was that "the great majority of these A.D.C. children have made remarkably good adjustments. . . . The indications of anti-social behavior . . . were very slight indeed. On the other hand the evidence of real accomplishment . . . in the face of great handicaps was strong." (p. xxii) A difficulty recognized by the authors consisted in the absence of a control group, though occasionally the data for A.D.C. families were compared with data for the general population.

But despite the progress made by the program the authors find that in many areas it is inadequate. The families do not have an income which would provide for the children nourishing diets, decent housing conditions (only slightly over one-half of the households had all three: electricity, inside running water, and private inside flush toilet), educational and other necessities for adequate development. Low income was associated with low educational status and children who were retarded in school were found more frequently in rural areas, were more poorly housed, more apt to be delinquent or neglected, and had received poorer health care. Apart from the need of greater financial support the authors urged better trained and less over-loaded caseworkers to help the families with their problems.

MIRRA KOMAROVSKY

Barnard College, Columbia University

The Stepchild. By WILLIAM CARLSON SMITH. Chicago: The University of Chicago Press, 1953. 314 pp. \$6.00.

There are stepchildren in scientific thought just as there are in real life. Mr. Smith has called all of us to account for our neglect of the stepchild in the sociological literature, with special mention of the present reviewer.

This volume is a determined and systematic effort to fill this gap. Like all Gaul, it is divided into three parts. The first one deals with the background of the problem, showing how popular usage makes the term a synonym for neglect, how folklore in various countries identifies the stepmother as a symbol for cruelty, particularly those in the Euro-American area. In drama, in fiction, and in biography, the story is much the same, a story of neglect.

Part II deals with the stepparents, pointing out the basic difficulties which the relationship presents. Most of these concern the stepmother. Her role naturally is a difficult one, calling for the wisdom of the serpent and the harmlessness of the dove. In addition, there are the complexities which our culture adds, in contrast to those of more primitive peoples, and the stereotype which it presents of her as being lacking in consideration and motherliness. "Our culture defines her first and then makes her fit the definition." Meddling outsiders still further complicate the problem, so that the stepmother who succeeds in developing bonds of affection with her stepchild does a truly remarkable job. Stepparents are a part of the problem, too, and vary in kind as do stepmothers, but their difficulties tend to be less numerous and serious.

The final section of the book is constructive in tone and deals with the adjustment problems of the stepchild. Age is shown to be a factor, the difficulties increasing with age, with adolescence the high point. They are greater at the lower socio-economic levels. The nature of the intermediate care of the stepchild, i.e., the time between the death of the parent and the advent of the stepparent, is highly important. Many grandparents have far from a clean slate in this respect. Much depends on the degree of idealization of the departed parent, as well as the nature of the relationship between the remaining parent and the stepparent. To the reviewer, the chapter on the preparation of the stepchild for the coming of the new parent is particularly important and suggestive. The basic factors in the problems of adjustment are emotional and attitudinal, and to these the author accorded due stress.

Professor Smith has obviously devoted a tremendous amount of research to this study. The data and case studies presented cover a wide area and a number of years. Numerous cases are cited which add clarity to the exposition. The style is easy to read and the material is well organized. All in all, this book is a distinct contribution to an important yet overlooked human problem.

JAMES H. S. BOSSARD

University of Pennsylvania

The Child's Conception of Number. By JEAN PIAGET. (Translated from the 1941 French edition by C. GATTEGNO and F. M. HODGSON). New York: The Humanities Press Inc., 1952. ix, 249 pp. \$5.00.

Piaget has been known in this country since the nineteen-twenties for his books on child thought. The translation of these books provoked a spate of studies utilizing his concepts and checking his assertions. Opinion varies considerably among social and child psychologists concerning the validity of his findings. Apparently undisturbed by the American literature bearing upon his work—for he never refers to it—Piaget continues to publish on one phase or another of child thought. Until 1950 none of this later research was available in English, nor was it known in this country to judge by the general silent treatment given it in the bibliographies.

The Child's Conception of Number probably ought not to be tackled before a reading of *The Psychology of Intelligence* (translated in 1950). In the latter volume Piaget's conceptions of child development are outlined in detail. The idea of stages is central to Piaget's scheme. The research task is to characterize each stage of development of thought and action, and to describe the transformation of the child's behavior as he moves to progressively higher stages. Transition from one level to the next is marked by successively more complex coordination of responses and by an increasing differentiation of response. Early infant behavior is conceived of as exhibiting various types of sensorimotor activity (described in detail in *La Naissance de l'Intelligence*; in *La Construction du Réel chez l'Enfant*; and in *Play, Dreams and Imitation in Childhood*). With the beginnings of speech, sensorimotor behavior passes into the stage of preconceptual thought in which there is no real logic of classes or relations. From the age of four to seven or eight, the child thinks "intuitively." He reasons, but exhibits many varieties of faulty logic. He makes mistakes because he fails to take multiple possi-

bilities and perspectives into account simultaneously. He takes appearances for granted and this traps him. His images and anticipations still are linked with his own immediate actions and so lead him to wrong conclusions of an egocentric nature. Experience with manipulable objects leads the child eventually to correct his intuitive concepts and he passes then to a stage marked by correct logical operations. But these are correct only providing the objects reasoned about are concretely in front of the child. By eleven or twelve years of age, logical operations are adequate even in the absence of objects.

Piaget has published a series of monographs dealing with the child's gradual acquisition of socialized concepts of space, quantity, speed, time, and number. The aim in each volume is to trace in detail the level-by-level development: from the sensorimotor stage to the rational coordination of thought by formal logical operations. *The Child's Conception of Number* is an early volume from this series. Its main thesis is that "the construction of number goes hand-in-hand with the development of logic, and that a pre-numerical period corresponds to the pre-logical level . . . gradual elaboration of systems of inclusions (hierarchy of logical classes) and systems of asymmetrical relations (qualitative seriations)." Logical operations and arithmetical operations arise simultaneously and interdependently. As always in Piaget's books, the point is made that logic is learned, is not innate, and therefore that children's early thought processes are not identical with those of adults.

Detailed protocols are given showing the child at work handling test materials and answering the questions directed at him during the test situation. The tests are ingenious and varied. The children's remarks, naturally, often make delightful reading, and their errors provoke chuckles. Piaget's own style is laborious and the translators stick meticulously by it. The research is systemic, chapter building upon chapter. Social psychologists will not find the subject matter particularly close to their interests, but the careful tracing of conceptual development suggests clues for studying the learning of more "social" concepts.

ANSELM STRAUSS

University of Chicago

Paths of Loneliness: The Individual Isolated in Modern Society. By WOOD, MARGARET MARY. New York: Columbia University Press, 1953. Pp. x, 250. \$3.75.

A book of this type is long overdue. Through case studies, behavior specialists are

aware that a great many persons do not feel their essential relationship to others. One hopes that Professor Wood will continue her research in this field and fill the many gaps in this commendable pioneer study.

Part I deals with "forces in society which tend to isolate one man from another." Part II is designed to analyze some of the ways in which lonely individuals react to isolating processes in their efforts to relate themselves to others. Chapter headings are selected from the terminology of important writers like Matthew Arnold, Rudyard Kipling and Francis Bacon. In Chapter I, "The Uncompanion'd Way", "sundering powers" such as race, religion, nationality, class and caste are revealed. Earlier studies are briefly reviewed.

Each chapter heading is mentioned by name in the order in which it appears. "Single Men in Barricks" deals with the segregation of the sexes in military and civilian life. "Men of a Different Generation" shows, among other things, the gap between generations with some emphasis on aging. "Men Without Work" stresses humiliation. No recognition is given the fact that some fear employment more than unemployment. "Men in Great Place" deals with the loneliness of command. "Men Beyond the Pale" is a brief view of social variants and refugees.

"Victims of the Evil Eye" has to do with superstitions and fears concerning leprosy, venereal diseases, mental ill-health, deformities, blindness, deafness, harelip, and birthmarks. "The Far-Wanderer" discusses explorers, seamen, shepherders and others in terms of loneliness. "The Lonely Egotist" makes a flight from reality into neurosis, alcoholism, drug addiction and suicide. Nothing is said about the fact that each person, even the altruistic person, is both egocentric and socio-centered. There are solitary egotists and extreme egotists who see much of others. The "Authoritarian Escapist" who does not feel his relatedness to society becomes a follower of a dictator with authoritarian techniques. Are these people lonely with all the collective protection of other followers? Was the true Nazi lonely?

"Who Cannot Mend His Own Case" may impair the good fortune of others and remain detached. However, if he is of a "brave and heroic nature" he will do something of a high order and gain recognition. "The Brave and Heroical" may become a genius or do an ordinary thing better than others and bring hope to others fighting against frustration and loneliness. These chapters are followed by an inadequate conclusion which covers only a page and a half. There is a short bibliography.

All sociologists and especially all social psychologists should read this book. Social workers will find it useful. One feels that Professor Wood could have turned this valuable book into a great book if she had handled her materials in a sociological or a social psychological frame of reference. Little effort has been made to show the inextricable relationship between the person and society. A person is nothing apart from society. The lonely person fails to see this. His pathology lies, in part, in the fact that he does not understand that he is nothing apart from others. A person spends all his life getting into society and getting society into his own life organization. A person is society, a particular view of society. Even loneliness is a product of this process. There is collective protection for loneliness.

The lonely person needs to know that he is not only escaping from others but is also escaping from himself, from his real self, developed in interaction with others. Loneliness may bring fear but it may eliminate self-contempt and self-accusations that would function in the presence of others. Loneliness is a beneficent adjustment for those who want to lose touch with their actual selves. It may be a guard against frustration or some other pathology. Contacts with many force one to see himself as a whole, with little chance for a partial elimination of self.

There is no scientific evidence to show that persons with infrequent contacts have more frustrations and are less happy than persons with many contacts. People who have experienced loneliness may want to continue in isolation. Loneliness, like any other pattern of behavior, tends to maintain itself. People who have not been liked may not want to be liked. Loneliness is self-protection in which no one gets a hold on the person. Some persons prefer strangers to friends and relatives. Attachments are avoided if one is afraid of intimate involvements that create frustrations because of demands. The lonely person may want to change but one can be sure that he also does not want to change.

One finds the same type of "stability" in "abnormalities" that he finds in "normalities." There is stability in loneliness—the same type of stability found in frequent contacts. The social deviant is often the product of many contacts and then feels lonely because he is a deviant. Mental illness has group corroboration and is often, perhaps always, an exaggeration of the prevailing mood in the group. Dichotomies are misleading. One finds in intimacies the same pathologies found in loneliness.

L. GUY BROWN

University of Rhode Island

Social Psychology. By ROBERT E. L. FARIS. New York: The Ronald Press, 1953. vii, 420 pp. \$5.00.

This is a clearly written, highly readable text that falls squarely in the distinctive and distinguished tradition of American social psychology which emerged in the works of James, Baldwin, Dewey, Cooley, and Mead. Basic theoretical emphasis is placed on the primacy of the act, the interactional origins of consciousness, language, thinking, and the reflexive self. The dynamic social environment is held to be a major determinant of the content and quality of man's psychological processes: attention, perception, learning, memory, thinking, motives, attitudes, and the self. The crucial contributions of social interactional processes to intelligence, special abilities, neuroses, and other personality disorders are given special treatment. A final chapter outlines the author's interpretation of the main trends and principal problems of contemporary social psychology.

Its content and many good qualities make of the work an excellent antidote to the teaching still found in some of the more backward behavioral science departments—not limited to "undeveloped areas" but happily a declining population.

With all of its virtues, however, it must be admitted that the book falls disappointingly short of what it could and should be. The writers in the tradition the author here represents have relied much too long on an anecdotal discursive style of communication with a consequent looseness of formulation of their problems. The present author has regrettably prolonged that style which, while it communicates effectively with those readers who have remained content with it, no longer satisfies the current need for more rigorous and systematic organization of hypotheses and evidence. It perhaps should be noted parenthetically that the work is somewhat deceptive in this respect. It is more rigorous and empirically buttressed than appears on the surface.

In the second place the book is unnecessarily controversial in tone and unfortunately carries on its arguments with the somewhat obsolescent psychology of the older textbooks. A slim justification of this might be that the author is writing for students who will be getting that kind of psychology. But this is not enough to counterbalance the disadvantage of failing to communicate with those who are or can be seriously interested in an interactional approach. To have the proper impact on these readers the interactionists must go much further than they have in a more explicit structuring of their problems in terms of "middle range" hypotheses

and in developing designs and methods for their testing.

This book makes it much too easy for the more bigoted among the high priests of psychology to drag out their stale but nonetheless valid observation that workers in the general theoretical framework represented by this book have failed to produce any substantial experimental verification of their hypothetical position. It might be noted in passing that apparently it has never occurred to these critics to turn their own talents to testing interactional theory. The chief reason for this reluctance is that to tackle this field, investigators have to break out of thought patterns and methods which have given them a comforting sense of security and which they thus want to hold inviolate. A past president of the American Psychological Association of recent vintage, in his presidential address, vigorously and courageously called for a drastic shift in psychological theory from a monadic to a dyadic orientation. Thus far there is no great rush among the mighty to take up his challenge.

However, in what are sometimes spoken of as the less honored areas of psychology there are signs of a tardy but vigorous interest in developing a thoroughgoing interactional psychology. To this task the younger scientists are bringing not only excellent discipline and training but also a zest for attacking the complexities of interactional problems and for developing more suitable methodologies for their investigation. On the sociological side this movement is matched by a growing number of well trained investigators thoroughly oriented to an interactional approach and ready to collaborate with their colleagues from psychology in building and testing a theory of social interaction adequate for the needs of research in individual as well as collective behavior. It is for these new explorers that future works in social psychology must be written.

LEONARD S. COTTRELL, JR.

Russell Sage Foundation

Psychoanalytic Theories of Personality. By GERALD S. BLUM. New York: McGraw-Hill Book Company, Inc., 1953. xii, 219 pp. \$3.75.

That the writings of Freud and his followers will perforce contribute heavily to the formulation of an adequate theory of personality has long been recognized. The present volume is an effort to review some of the psychoanalytic materials which may well contribute to the construction of such a theory. The author has read widely and has done a good job in digesting a vast array of publications in the field.

The author divides his treatment between the

echt Freudians and those called "neo-Freudian." In addition to Freud himself, the *dramatis personae* of the former include, among others, Anna Freud, Otto Fenichel, Richard Sterba, Phyllis Greenacre, and Heinz Hartmann. From the latter the author refers chiefly to Karen Horney, Erich Fromm, Clara Thompson, Abram Kardiner, and Harry Stack Sullivan. Then, too, there are psychoanalysts like Erik Homburger Erikson and Franz Alexander who do not easily fall into either camp. Of course, it is well to recall earlier secessions, and Blum does give some space to the defections of Adler, Jung, and Rank.

In presenting, first, the orthodox and, then, the deviant Freudian theories, the author has organized his discussions along a "chronological sequence of personality formation." He begins with "prenatal and birth influences" and ends, seven chapters later, with "adult character structure." For the most part he has used a common subdivision of content for each level. These cover such matters as ego and superego formation, psychosexual development, relationships with other persons, and the well-known psychoanalytic mechanisms: identification, projection, and so on. While such a temporal orientation has some advantages and while it is true that Freud himself used a chronological sequence from infancy, latency, adolescence and adulthood, nevertheless this organization tends to make for considerable and at times somewhat dull repetition of material. That a chronological frame of references is not the only possible one is evident in Healy and Bronner's *The Structure and Meaning of Psychoanalysis* (1930). They used a quite different though, on the whole, satisfactory organization.

Blum's treatment of the various phases of psychoanalysis is largely from the standpoint of traditional individual psychology. He seems to have little appreciation of the present-day stress in social psychology on interactional processes. Moreover, in view of current interest in cross-cultural data with respect to the factors which influence the development and function of personality his critical comments at times lack scope and depth. A good illustration of this is the failure of the orthodox Freudians to recognize cultural factors in treating adolescence. Furthermore, the neo-Freudians do not do much better. There still remains the task of drawing together social psychology, cultural anthropology, and psychoanalytic formulations in order to build a satisfactory theory of personality.

Yet these critical comments are not intended to detract from the value of this particular book. Within the framework of its organization, the author has performed a useful job of presenting the views of past and present-day

workers in the field of psychoanalysis. In addition the author makes many cogent remarks on the need to undertake research to verify or disconfirm the Freudian and neo-Freudian claims and theories. Blum's own investigations illustrate a good start in the direction of putting some of these ideas to empirical test.

KIMBALL YOUNG

Northwestern University

Sociology: The Science of Society. By JAY RUMNEY AND JOSEPH MAIER. New York: Henry Schuman, Inc., 1953. viii, 192 pp. \$3.00.

The editor said 400 words. I had 1200. As I started to rewrite, I vaguely remembered reviewing the book about fifteen years ago (*ASR*, Feb., 1939, pp. 121-122). Since the first review said about the same things and was briefer, I threw away the second.

The revision is seventy-five pages longer. An added chapter on "Worship, School and Play" accounts for all the major institutions except health, social welfare, and art. The entire book is better. It still is a marvel of condensation, concision, and clarity. It still is the best short answer to the long question, "What is sociology?" All beginning students should read it before the intensive study of a general text and most graduate students should read it the night before they face their ruthless committees.

The authors' viewpoint is well expressed by their quotation from Ward, "The real object of science is to benefit man. . . Sociology, which of all sciences should benefit man most, is in danger of falling into the class of polite amusements, or dead sciences" (p. 167). To them, sociology is a synoptic science attempting "to establish empirical generalizations or laws of change and growth in social life, and if possible to interpret them in the light of more ultimate laws" (p. 8). Value-judgments are inescapable for science and the basic one is that scientific procedures should be as objective and rational as possible.

I question whether the data of sociology are "more complex, numerous, unwieldy, and unreliable" than physical and biological data; whether there is any essential difference between the methods and purposes of the social and the physical-biological sciences; whether it is "foolish to expect precise measurements and exclusively quantitative analysis" in the social sciences (pp. 21-23). Absolutely precise measurement and exclusively quantitative analysis are of course impossible in all sciences. Practically all their illustrative material refutes the authors' anti-quantitative and pro-complexity position. The "cause and effect" problem is the same in all sciences. Objectively observable

actions of people, not "wants and desires," are the data of sociology.

They give too much space to the unsolvable problem of "social origins." They frequently use "determine" for "ascertain," "group" for "category," and make you fumble in the back of the book to find out who said what. They underestimate the seriousness of overpopulation (p. 110). It is nonsense to think Balzac (p. 121) or any other literary man has analyzed "the subtle and overt behaviors of the social classes" better than social scientists. Who knows whether the Industrial Revolution was "the most important series of events in man's history"? (p. 148) "Most important" is a dangerous phrase for sociologists. Is it true that most sociologists have been "conservatives" (p. 151)—whatever that means?

One could go on with minor—and some major—criticisms, but the conclusion should emphasize that this little book is a fine job of condensation without sacrificing accuracy or readability. It is a superb brief "overall-look" at sociology.

READ BAIN

Miami University, Ohio

Our World From the Air: An International Survey of Man and His Environment. By E. A. GUTKIND. Garden City, New York: Doubleday and Company, Inc., 1952. xii, unpagged, 400 aerial photographs. \$7.50.

Four hundred aerial photographs comprise this fresh analysis of man's relationship to his environment and his fellow men. Without doing more than turning the pages, and as if from an hovering helicopter, you can study the physical features of every type of human settlement along the rural-urban continuum. By carefully selected, representative examples from over the world, these photos-with-commentary provide a cross-cultural survey of the growing but still puny construction man has piled on limited portions of the earth. This is an "aerial history" of emerging settlement patterns.

E. A. Gutkind, known in the United States primarily by his two volumes in the International Library of Sociology and Social Reconstruction, spent several years in the development and presentation of this record of human ecology that no single individual could ever anticipate observing in person. A Foreword by G. P. Gooch indicates that the book was published under the auspices of the British Institute of Sociology. Lewis Mumford contributes a penetrating Introduction and Gutkind rounds out the preliminaries with a useful and provoking discussion of "Man and Environment."

This book is divided into four main parts, each dealing with a successive stage in man's reaction to, and modification of, his environment. Part I (Fear and Security) includes photographs on displacement and replacement of nature by man, temporary shelter, villages, site and protection, castle towns, countries as defense units, and protection by retirement and solitude. Part II (Confidence and Adjustment) depicts examples of replacement connected with water, adaptation and replacement for agriculture, adaptation by rational use and technical skill, cities and towns, villages and towns of distinctive character, and walls, squares and houses as unifying factors. Part III (Aggressiveness and Disintegration) illustrates fixed industries, industry and living place, rural isolation, and unsystematic expansion. Part IV (Responsibility and Unification) presents typical samples on dams, the new fields, industry decentralized, the beginning of new town planning, traffic as a unifying factor, and conquest of the air.

The annotations for the photographs enhance their value; it is regrettable that the notes are not longer, but the photographs in themselves actually require little aid in proving the old formula that each is equal to thousands of words. Skillful juxtaposition and contrast underline relationships that might otherwise be overlooked.

Claiming that a book has value for several different publics is usually tantamount to damning it. In this case, however, there is no doubt that this integrated collection of photographs will have considerable use by sociologists, anthropologists, historians, human geographers and human ecologists, as well as urban planners. As Lewis Mumford points out, "Not the least service that *Our World From the Air* performs is to deliver us from bondage to the map," at best an abstraction often less enlightening than a photograph. Use of the volume for instructional purposes is facilitated by a Reference Map, a geographic index, and a Subject Index, all keyed to the photograph numbers.

This particular book covers the world for its subject matter. A very few like it are concerned with particular countries, only, and are mostly out of print or otherwise inaccessible. The technique might profitably be tried again for perhaps more specific or limited subjects as a partial antidote to our chronic earth-bound perspectives.

GERALD BREESE

Princeton University

Struktur und Funktion von Landgemeinden im Einflussbereich einer deutschen Mittelstadt. By HERBERT KÖTTER. xv, 182 pp. *Landbevölkerung im Kraftfeld der Stadt.* By KARL-GUENTHER GÜNEISEN. xvi, 113 pp. *Der Nebenerwerbslandwirt und seine Familie im Schnittpunkt ländlicher und städtischer Lebensform.* By GERHARD TEIWES. xvi, 196 pp. Darmstadt: Eduard Roether Verlag, (Gemeindestudie des Instituts für Sozialwissenschaftliche Forschung, Darmstadt, Monographie 1, 2, 3), 1952. No prices indicated.

These are the first three of ten monographs dealing with Darmstadt and four communities of its hinterland. They were sponsored by the Darmstadt Social Science Research Institute, originated through the initiative of Nels Anderson. The German sponsor of the surveys was the Akademie der Arbeit at Frankfurt University. S. Earl Grigsby, HICOG Office of Labor Affairs, Henry J. Meyer and H. Ashley Weeks, New York University, T. W. Adorno, Institute of Social Research at Frankfurt University, and Max Rolfes of the Giessen Institut für landwirtschaftliche Betriebslehre, served as major consultants. Rolfes and Adorno wrote far-ranging prefaces to the three studies under review, and Grigsby (Monographs 1 and 3) and Anderson (2), very good English abstracts.

Kötter's volume, *The Structure and Function of Rural Communities in the Area of Influence of a Middle-Size German City*, deals with the relations between Darmstadt (pop. in 1939, 115,000; in 1945, 56,000; in 1950, 92,000) and Alsbach (pop., 2,316), Spachbrücken (1,598), Schneppenhausen (973), and Ober-Mossau (526), which are, respectively, a residential community, one between a worker's residential and a worker-peasant community, a workers' residential community, and a worker-peasant community; or, in Grigsby's terms, a small rural town, an interstitial rural community, a satellite community, and a rural farm community. They were selected out of 217 communities in the region (Teiwes, p. 144) according to "type of rural settlement, land use, socio-economic structure, population characteristics, transportation and communication facilities, and distance from the city" (Kötter, p. 165). The study describes the process of urbanization in terms known from this country. The process is the more conspicuous, the closer its locale is to the city; it is characterized by reduced isolation and homogeneity (in occupations, especially in the increasing impact of non-rural occupations—

a topic to which Monograph 3 is devoted; in religion, especially in the intrusion of Catholicism, through evacuees and DP's, into the predominantly Protestant region; etc.); by industrialization; by the diminished significance of the family, neighborhood, and church; by the rise of secondary groups; and by the diffusion of mass culture.

The function of Kötter's volume seems to be to provide part of the background for the other studies, especially facts on the four communities. If so, some of its content is superfluous, as, e.g., an announcement, with picture, of Dodd's population-space-time vectors, erroneously said to provide the framework for the research. In fact, the monograph is an interesting exhibition of the impact of American sociology on German social inquiry. Texts and studies in rural sociology are used (Chiefly Landis, Loomis and Beegle, T. L. Smith, and C. C. Taylor), and several concepts, such as "primary" and "secondary" groups, and research techniques are taken over. But the American influence has not eliminated all cultural beliefs as elements intrinsic to the conceptualization of the enterprise. Less subtle instances are certain romantic conceptions of the peasant ("It may sound heretic to assert that real peasantry no longer exists nor can exist. . . . To be sure, it will not be denied that the relation to the earth and the life of creatures gives the peasant a special place in the great whole of earthly life" [p. 90]); the denial of a rural proletariat (not defined) (p. 109), an assertion at the very least qualified by some observations contained in Grüneisen; and the operation with an unquestioned standard of proper domesticity ("... beside a wonderful hand-carved grandfather's-clock hangs perhaps an ugly color-print, or cheap knick-knack figurines stand on beautiful old pieces of furniture; in short, the atmosphere of the house is not one in which a *gemütliches* family life can unfold" [p. 129]).

On the other hand, the volume, aside from its merits on the factual level, contains several suggestions worth following up. Thus, there is a historically oriented classification of rural occupations which may well be tried elsewhere: (1) those which are the first to appear upon the dissolution of the old household economy and which require technical training or equipment that cannot be borne by the individual household; e.g., locksmithing, tailoring; (2) those which emerge with increasing division of labor but are still largely dependent on the local village market; e.g., butchering, baking, small business; (3) those depending on cus-

tomers outside the village; e.g., industry, wholesale. These three developmental occupational categories are cross-classified with four "horizontal" divisions: agriculture and forestry, production, commerce, and public and private service (pp. 66 ff.). Another significant observation concerns a circumstance opposed to the process of urbanization: the recent war has caused resentment against the farmers "who often are reproached with a lack of social sensitivity," while the peasants tend to consider urbanites as lazy (even though it "could be observed that this mutual misunderstanding did not appear as strongly in the communities closer to the city as in the more rural hinterland") (p. 95). Finally the volume contains an observation about a rising new middle class which suggests similarities with processes within our own social structure ("while collar"): "Instead of the old division into farmers and workers, there arises a new middle stratum in which may be counted small businessmen, including small farmers; craftsmen; small officials and employees; and finally, skilled workers" (p. 107).

Grüneisen's *Rural Population in the Urban Field of Forces* attempts to measure the degree of urbanization by applying two scales to a sample drawn from the four communities. The scales try to ascertain degrees of conservatism (or "oldfashionedness") and modernism. Their construction, the constitution of the samples, and problems of their comparability are carefully described. The scales contain both objective (family composition, presence of modern appliances, etc.) and subjective items (attitude toward church, divorce, the industrialization of the community; interpretation of Marshall Plan; opinions on aims of education, etc.). Scores were distributed on a five-point continuum, running from most conservative to most modern, and were related to father's occupation and social position, and subject's place of birth, education, occupational training, military experience, occupation (main activity, social position, place of work), and native, refugee, or evacuee status. Occupation has the greatest significance (correlations are not given) for the place on the conservatism-modernism continuum; place of birth and education follow next; then place of work and native-refugee-evacuee status. Statistics are followed by descriptions of five cases, each representing one of the five groups in this order of decreasing conservatism (increasing modernism): a farmer with an additional occupation, a refugee, a farmer-carpenter, a village teacher, a mechanic. These cases show the significance as well as

the insufficiency of the statistical indices. To cover more aspects of the process of urbanization statistically, some composite index might have been useful.

As in so many rural-urban studies, ruralization is recognized, that is, urbanization is admitted as a two-way process; but the question of why it actually is almost one-way in favor of the city is hardly faced. There is however, a relevant hint which ought to have been followed up:

Evidently, the rural environment, even in cases of direct contact, is not as able to ruralize in the same measure in which the city, inversely, urbanizes even those rural people who are only indirectly under its influence. The reason possibly is that vis-à-vis [*gegenüber*] urbanites, rural persons have lost their healthy self-reliance because in economic and political discussions they get [pushed] ever more into the defensive (pp. 40-41).

The three monographs contain very little on refugees, but a passage in the case study of the refugee found in Grüneisen may give a clue to a more general feature of refugee-native relations: it is suggested that natives and refugees with like social origins and occupations nevertheless show no solidarity because of their different experiences: "We refugees alone seem to have lost the war—anyway, this is the way the natives act" (p. 72). The possibility of the refugees' political radicalization (and the light this process throws on the meaning of "urbanization") is apparent and is pointed out by the author. (Cf. this against Kötter on the rural proletariat, above.)

Volume 3, *The Part-Time Farmer and His Family at the Intersection of Rural and Urban Life*, deals with the various combinations of agricultural and non-agricultural occupations found in the four communities. The addition of a non-agricultural to an agricultural occupation is not new but has greatly increased in recent years. "Part-time farming" is defined as an agricultural enterprise whose head is or was (hence the inclusion of invalids, pensioners, and widowed in this study) engaged in a full-time non-agricultural occupation (p. 11). Twenty-one per cent of all families examined in the four communities (p. 30) managed an agricultural enterprise subordinate to their non-agricultural occupation, that is, were neither full-time farmers nor non-farmers. Major types among them were agricultural and forestry workers; rural craftsmen and businessmen, including inkeepers; commuters; and various others, with pensioners most conspicuous in this last group.

The first part of the study is a detailed economic analysis, including a breaking down of the utilization of working capacity by size of holding (p. 54 f.), which shows a trend from incomplete utilization to overburdening with increasing size, the cutting point being at about 1 *hectare* (2.5 acres) but varying in the four communities from half to over two. The second part is devoted to a description of miscellaneous sociological characteristics of predominantly conservative or modern outlook (ascertained by means of the scales detailed in Grüneisen). As might be expected, full-time farmers were found to be most conservative, non-farmers most modern, and part-timers in-between but approaching the latter (p. 72). Among the part-time farmers, the order of increasingly modern outlook ran from agricultural and forestry workers through rural craftsmen and businessmen, pensioners and widowed, commuters, to remaining part-time farmers (p. 74). Despite the commuters' greater urbanization in other respects, expenses for movies, theaters, newspapers, magazines, coffee, etc., were found to be no higher among them than among the other brackets (p. 115 f.), and the percentage of them that said they never attended church was lower than in any other group (p. 119 f.); but no satisfactory explanations are given for these phenomena. The over-all conclusion of the study is that part-time farming is a lasting occupational arrangement. As Crigsby points out in the English abstract, however, one of the problems not sufficiently explored is

the efficient use of land by the encroachment of part-time farming. Besides the inefficient use of manpower, many of the part-time farmers do not make full use of their land and many cease their farming activities when their off-farm incomes are sufficient to meet their standard of living requirements. Unfortunately, the land frequently lies idle and this adds up to a considerable loss to the total German economy (p. 186).

The three monographs show, for the most part implicitly, concern with the evaluation of the trends observed, especially with the question of whether they lead to a synthesis between city and country superior to either way of life. Kötter (p. 92) speaks of the capacity of the peasantry to adapt to the "historical situation"; this adaption will be furthered best through more education—the position sounds familiar. Less piously, Grüneisen's case from Group 3 (midway between "most conservative" and "most modern"), of a farmer-carpenter, suggests perhaps, according to the author, "a step toward the future form of

fusion between rural and urban forms of life" (Grüneisen, p. 78); here, reference is to the combination of agriculture with a carefully planned development of carpentry, including the introduction of machinery, and to an expanding political and economic horizon. The city, however, means not only such increase in functional rationality but also "mass culture," which is both the first manifestation if not the whole meaning of urbanization, and an anti-rational force. The studies furnish some raw data bearing on the fundamental problem involved here, but do not tackle it.

KURT H. WOLFF

The Ohio State University

Survey of the Social Sciences in Western Germany. By MAX HORKHEIMER. Washington: Library of Congress, 1952. ix, 225 pp. \$1.00.

The present report is a sequel to two earlier surveys of the social sciences in Western Germany, written by Dolf Sternberger. Strictly speaking, it is a survey of German sociology and social psychology rather than of the "social sciences." Dr. Horkheimer's report consists of a survey of teaching facilities in sociology and social psychology, of publications in these fields, of "current sociological activities," and finally of a detailed bibliography which makes up the bulk of the volume. Since this survey is primarily a reference-work, it is perhaps sufficient to describe its content generally.

In surveying West German teaching facilities Dr. Horkheimer identifies at each academic institution the professor who teaches in the field of sociology and social psychology as well as the specific topics on which he lectures and does research. Although interest in the field is growing, resistance to its independent academic status continues unabated with the result that its development occurs at the instance of individual academicians and in the interstices of the official curricula. Horkheimer also characterizes the principal publications containing contributions of sociological interest as well as the content of doctoral dissertations, which unfortunately are no longer made available in printed form. Under "current sociological activities" an annotated survey of current research-projects is presented which gives an excellent picture of the investigations under way at the present time. The extensive bibliography of German writings (including articles in periodicals) has been annotated. There is also a useful index of names and topics.

It may be added that Dr. Horkheimer's survey impresses this reviewer in two respects.

It shows that the older tradition of German sociology, identified with such names as Weber, Tönnies, Simmel, Mannheim and others, is no longer alive. It shows also that American sociology and social psychology have become a most important intellectual influence. Perhaps neither of these conditions is permanent, and it is quite true that considerable activity in sociological teaching and research is under way—despite the unfavorable institutional setting. It remains to be seen, however, whether German sociologists will be able to synthesize the German with the American tradition. They are certainly entitled to a sympathetic interest and encouragement by their colleagues abroad in view of the lapse of an entire academic generation during the Nazi-period.

REINHARD BENDIX

University of California, Berkeley

Soziologie: Geschichte ihrer Probleme. By HELMUT SCHOECK. Munich: Karl Alber, Publisher, 1952. x, 431 pp. DM 26.

"Orbis Academicus" is the title of a series in which genetics and historiography already appeared and now sociology is considered in this new way of presenting a history of problems in documents and commentaries. This new kind of textbook is a very valuable attempt at a history of sociology—for German schools. It is written by a professor at Fairmont State College, West Virginia and it deals with German sociology up to 1933 in a very intelligent survey which is in some ways a sociology of sociology. The chapter on the sociology of knowledge is therefore most important, showing the tendency of this textbook to give not merely a chronological and biographical account, but also the historical context in which the problems arose and developed. The author has not ordered the documents according to the separation of German and French or Western sociology. The French sociology, beginning with Saint-Simon, and what is called French "social psychology" in which Tarde and Le Bon are treated together with Durkheim, is very poor; and the American sociology selects only MacIver, Merton, and Parsons, and mentions Ross, Park, Burgess, Thomas, etc. in one line. Giddings comes after Durkheim as a "social psychologist" and Lester Ward is mentioned in a short preface to the formalist school of Simmel (who is twice [pp. 212, 221] confused with the designer Paul Simmel). The sociology of the Russians, the Rumanians, and Czechs, as well as the sociology of Italy and Belgium are not even mentioned. The bibliography (45

pp.) and also the personal index (20 pp.) are a very great help for further studies.

In the German view of sociology romanticism (as in the school of O. Spann) plays a very great role, but it is not to be distinguished from the historical school. While the author recognizes natural law philosophy as a source of social thought, the scarcity of documents is astonishing. It would be understandable if the author limited the field to the civil society, like H. Freyer, but the whole problem of social reform is left out and the role of Lorenz von Stein as the transmitter of the problems of French reform and revolution is not seen in its impact on Marxist sociology. In a history of problems and the interpretation of the social process it seems necessary to make a clear distinction between antiquity and modern times. However, in spite of his own doubts (p. 54) the first part of the document deals with Greek and Christian antiquity. The critical remarks about many interpretations of the texts are not meant to deny the value of this work which is not the first, but a new way of presenting a history of social ideas. The publisher pretends that it is the first history of problems of sociology, which it is not.

GOTTFRIED SALOMON DELATOUR
New York City

Reader in Bureaucracy. Edited by ROBERT K. MERTON, AILSA P. GRAY, BARBARA HOCKEY, HANAN C. SELVIN. Glencoe, Ill.: The Free Press, 1952. 464 pp. \$5.00.

This Reader is "intended for the convenience of students of organization." No doubt this assemblage of 54 papers, three of which have not previously been published, will serve this purpose. The readings are organized around seven major themes: theoretical conceptions, bases for the growth of bureaucracy, power relations, structure, recruitment and advancement, the bureaucrat, social pathologies of bureaucracy, field methods for the study of bureaucracy. Each of these divisions is preceded by a brief editorial note that indicates the relation of the subsequent readings to the particular topic of the section. Thirty-nine different writers are represented in these 54 selections. Among the older writers Weber, Troeltsch, Veblen, and Michels are more particularly represented, and among the more recent American writers Simon, Bendix, Selznick, Gouldner, Gerth, Barnard, Roethlisberger, and Dimock are each represented by two or more selections. The three previously unpublished papers are by Carl J. Friedrich who has written a somewhat petulant critique of Weber,

Frederick S. Burin who contributes a valuable discussion of German bureaucracy under National Socialism, and Alvin W. Gouldner who has written on bureaucratic red tape. A useful bibliography concludes the volume. Since almost all the readings are drawn from well-known sources in the field, the reviewer feels that he may indulge in the luxury of discussing the pedagogical questions raised by the volume rather than the individual papers.

Indeed, the very fact that almost all the selections come from readily accessible and well-known sources raises the question whether this volume, useful as it is, could not have made a still greater contribution by using rather different principles of selection. A reader increases in pedagogical value according to the degree to which it brings to the attention of students valuable material to which, for various reasons, they are least likely to have access. Most of the writers, books, and journals represented in the present volume are already well-known to moderately advanced students of sociology and are generally readily obtainable even in libraries of restricted scope. The increment of convenience provided by the Reader is therefore relatively modest.

More important, however, is the fact that the present set of readings does not provide the student with sufficient descriptive and historical material on bureaucracy to give the more abstract discussions in the sociological literature concrete significance. This is not due to any oversight or negligence of the editors. They clearly indicate in the Introduction that they have considered and deliberately rejected the inclusion of historical, descriptive accounts. They felt that this was not practicable, but surely a volume of 500 pages devoted primarily to historical materials which sociology students are less likely to find for themselves or have prior knowledge of would have provided sufficient space to have put considerable flesh and blood on the students' more abstract conceptions of bureaucracy. There is probably no shortcoming more evident in sociological instruction than the tendency for students to interest themselves in imposing analytical apparatuses without a prior or accompanying study of the concrete forms of social life which this apparatus is intended to order. Indeed, schemes of analysis and general ideas ought to grow out of a knowledge of the material to be analyzed and the student who forgoes the acquisition of descriptive and historical knowledge is simply not in a position to judge the value of more general analyses or to benefit fully from them.

An additional difficulty with the present

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Reader is that many of the papers, although chosen from works and writers with strong interests in organization and bureaucracy, are nonetheless tangential to the themes under which they have been placed. Thus the selection from Troeltsch, which appears under the topic "Bases for the Growth of Bureaucracy," deals primarily with Troeltsch's distinction between sect and church. There is virtually nothing in the selected passages that deals either with the conditions that led to the development of a church bureaucracy or that tend to be inimical to bureaucratic development in the earliest stages of sectarian growth. Similarly, the selection from *The American Soldier* dealing with differences between officers' and enlisted men's attitudes throws little light on "Status Systems and Gradations of Prestige." Similar remarks can be made about a number of other selections.

Every anthologist is no doubt prepared in advance for dissenting opinions on the selections that he has found most relevant for inclusion. The editors of the present volume would undoubtedly have found it impossible to prepare a reader that would meet the varied preferences that instructors in this field undoubtedly have. They have, nonetheless, provided a useful volume.

HERBERT GOLDHAMER

The Rand Corporation
Washington, D.C.

Science and the Social Order. By BERNARD BARBER. Glencoe, Illinois: The Free Press, 1952. xxiii, 288 pp. \$4.50.

"Science" means many things, and many disparate things, to many different people. It is at once a symbol and a shibboleth—an authority for the advertiser, a genie to the housewife, a tool to the industrialist, a weapon to the soldier, a danger to the moralist, an annoyance to the humanist, a fetish to the educator, a method to the philosopher, and a social institution to the sociologist. The author of this book, of course, is a sociologist, and he is concerned accordingly with science as a social institution. The measure of his competence may be discovered, however, in the fact that he can take account of some of these other views of science and integrate them into his general treatment.

Barber begins his book with a short introduction entitled "What is Science?" He walks on familiar ground here and reminds us again that science is neither facts alone nor theories alone but a combination of both. His discussion is brief and to the point—not, to be sure, an essay on the philosophy of

science, but a clear and orthodox statement of what science is about. He turns next to the historical development of science and considers the various social factors which have operated in its evolution, factors which are not wholly determinative because, as he believes, science has a certain autonomy of its own. Here he is a genuine sociologist of knowledge in the questions he asks and in the problems he poses for solution.

As if impatient with history, however, he moves rapidly into modern society, with chapters on the place of science in both liberal and authoritarian societies, the social organization of science in general, and the social organization of science in particular in American society. The central chapters of the book then deal with the scientist in three different places—in the university, in industry, and in government. The author is perhaps at his best in these chapters, and the facts which he adduces, together with the opinions in which he is candid enough to indulge, merit the most serious consideration.

Additional chapters deal with the social process of invention and discovery, and the social control of science. The book concludes with a chapter on the social sciences, and here he tells us what all of us would most like to hear; namely, that these sciences, as they mature, have a new and important role to play in government and in human affairs.

The book as a whole is a sensible and straightforward account of the social functions and the social consequences of science in our modern society. It should be read by everyone who has a stake in science, which is to say in society itself, for surely, as Barber indicates again and again, the role of science in this society can hardly be overestimated.

If one were to cast about for a criticism one would have to say that the author's interest in science is more social than sociological. That is, he presents his discussion in the framework of a social problem rather than in the framework of the sociology of knowledge. Although, as mentioned above, there is a short sketch of the history of science in an early chapter, the book as a whole is not distinguished by an historical orientation. With this single exception, and possibly one other, the discussion proceeds wholly on the level of present problems which are pressing for solution; such problems, for example, as what allocation of resources should be made between pure and applied science, whether the scientist should accept complete, some or no social responsibility for the consequences of his work, whether more planning or more freedom is desirable within science itself,

whether scientific activity should be formally or informally controlled, and so on. The intellectual locus of the study, in short, is institutional rather than ideological, the author's role is frequently that of publicist instead of sociologist, and the book in consequence falls somewhat short as a sociology of science. One raises this objection with reluctance—because the norms of reviewing require it, because Barber did not intend to solve this larger problem in this place, and because there is ample evidence here that he is fully capable of making a frontal assault upon it in his future work.

One cannot conclude without mentioning the long, thoughtful, and altogether excellent Foreword contributed by Robert K. Merton. With a brevity which makes for awkwardness one may say that, as an essay on the sociology of the sociology of science, it is itself an outstanding example—a paradigm—of the way the sociology of knowledge in general should be written. The Foreword indeed has more methodological and substantive implications than can be suggested here. If Barber had pitched his entire discussion on this level he would have had a rare addition to the sum of sociology. Even as it is, however, he has an important book.

ROBERT BIERSTEDT

The City College of New York

The Younger American Scholar: His Collegiate Origins. By ROBERT H. KNAPP and JOSEPH J. GREENBAUM. Chicago: University of Chicago Press, 1953. xiii, 122 pp. \$3.00.

This excellent account of a study conducted at Wesleyan University under a grant from the Fund for the Advancement of Education, is offered "as a straightforward and simple report on the undergraduate origins of younger American scholars who have won academic distinction in graduate school since 1946". It is a notable example of how an objective report of a well-designed study ought to be presented.

The methodological problems involved in planning such a study are complex and the authors have reported carefully both their definitions and the consequent limitations upon the study itself. A roster of younger scholars was compiled from four sources: 1. Ph.D. awards since 1948 in the twenty-five largest Ph.D. degree granting institutions; 2. winners of university fellowships or scholarships since 1946 in these same twenty-five institutions (but not including winners of assistantships); 3. fellowship awards since 1946 by the three major federal agencies having such a program. The study is further restricted to those who

received their baccalaureate in or after 1946 from colleges in this country, or 6,916 individuals. Male awardees were identified by collegiate origin in liberal arts, university, and technical subsamples; female scholars were studied separately.

The data show that a small group of institutions account for the bulk of the production of younger scholars. These are located principally in the New England and North Central regions. The highest cost institutions in all subsamples were found to be more productive than the middle range or low cost institutions. This holds for the production of scientists as well as for scholars in the social sciences and the humanities, a finding different from that reported in earlier studies. Privately controlled nondenominational institutions were found to be consistently high in production, those with Catholic affiliation very low. For the university sample, size of institution was found not to be a discriminating attribute but, for the other samples, a range of 100 to 200 male graduates per year was found to be optimum.

The statistical treatment is accompanied with a clear statement of its limitations as well as its applicability. With minor exceptions the text is clear and carefully presented. Tables are often compiled in terms of per cents but without N's, and table headings are omitted completely. An excellent "Discussion" (Chapter XI) summarizes the study and its limitations. This writer wonders only if the authors accredit the fact that institutions define their functions differently and that the production of scholars may be an objective secondary, for many institutions, to that of serving the more immediate needs of a certain area.

GLEN T. NYGREEN

University of Washington

The Uneducated. By ELI GINZBERG and DOUGLAS W. BRAY. New York: Columbia University Press, 1953. xxv, 246 pp. \$4.50.

In reporting to the readers of a professional journal on a book which was probably written for a different public, one runs the risk of seeming to ignore some of the book's intrinsic merits. But it has to be said here that, with the exception of a few chapters dealing with the experience of poorly educated men in the Army, *The Uneducated* contains little of either data or insight that will be new and significant to the average well-informed American sociologist.

This is the first monograph to emerge from Columbia University's research project on Conservation of Human Resources, of which the

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senior author is staff director. "All too long," say the authors, "the social sciences have been retreating from the lines of action." A scholar, they contend, "cannot escape his obligation to share with others his discovery and its bearing on their actions." Yet his role is left somewhat ambiguous, for he is warned on the same page that his advice may very likely be wrong: "The man who knows most about a subject does not always know best what to do about it" (pp. 222, 223).

The contents and argument of the book, leading to two recommendations for governmental action, can be summarized briefly: There are in the United States many adults who are illiterate or who have had less than five year's schooling, despite the fact that rates of illiteracy have trended downward and school attendance rates upward for many decades. These handicaps are more prevalent in the South than in the North and West, and their incidence is higher in the colored than in the white population. During World War II, 716,000 men were rejected for military service because of "mental deficiency," which meant in most cases lack of schooling. A small number of men were inducted into the Army in spite of this handicap, and were placed in special classes to learn the "three R's". In a sample of 400 of these men, the authors have found that about 6 out of 7 became "acceptable" and some of them "good" or even "very good" soldiers. Most of those who responded to a questionnaire after returning to civilian life stated that the special training had been helpful to them. Southern industrial employers report that they have not been seriously handicapped by prevailing educational standards in recruiting their labor forces; but in the Detroit area, for example, illiterate migrants are at a disadvantage in a labor market where literacy is the general rule. Many Navaho Indians and Spanish American immigrants in the Southwest suffer from lack of schooling. The Armed Forces ought to induct larger numbers of poorly educated men and teach them to read, write, and do simple arithmetic. The poorer States should receive Federal grants-in-aid for the improvement of their school systems. The book closes with a reaffirmation of the importance of full development and utilization of human potentialities.

An account of the organization and functioning of a singularly powerful type of collaborative research is promised in the Foreword: "There may be some basis for contending that the demonstration of how a project of this scope involving the active participation of so many organizations and individuals was designed and carried out is as important as the

presentation of the specific findings and interpretations" (p. xv). But what follows does not fulfill this promise. The statistics presented, excepting those derived from the special study of 400 soldiers, appear to come almost wholly from publications of the Census Bureau and Selective Service Headquarters. (It is necessary to say "appear to come," for exact sources are almost never cited.) Conversations and correspondence with officers of the Armed Forces, industrial executives, and personnel officers; "a visit to the city [of Detroit] in the spring of 1951;" and an anonymous "confidential report" on conditions in an unnamed southern county are typical sources of non-statistical information. It would seem that similar findings might have been produced by a man with an expense account and sufficient time, without benefit of a masthead listing a research staff of twenty members and without the prestige conferred by the sponsorship of thirteen well known industrial concerns.

It is announced (p. 225) that a second study of the Conservation of Human Resources Project will be reported under the title *The Ineffective Soldier*, while a third study will deal with persons of superior talent. If the purpose of these studies is to advance scientific knowledge of some very complex problems, it is to be hoped that the forthcoming publications will be less superficial than the present one. If on the other hand the main objective is to influence the makers of public policy, it will not be fair to judge them by the criteria implicit in this review.

ELBRIDGE SIBLEY

Washington, D.C.

Subregional Migration in the United States, 1935-1940, Vol. II. Differential Migration in the Corn and Cotton Belts. By DONALD J. BOGUE and MARGARET JARMAN HAGOOD. Oxford, Ohio: Scripps Foundation, 1953. vi, 248 pp. \$2.25.

This study is a significant substantive contribution to the field of differential migration. It is perhaps even more significant as a model and as a source of data for a set of potential related studies by other researchers.

The areas investigated are two of our important interstate regions; "The Corn Belt" and the "Cotton Belt." These areas are large enough so that the streams of migration involved are not trivial either in number or in their general social significance.

For each of these two regions, the study compares significant characteristics of intrastate migrants to urban places with the non-migrants at the origin and at destination points. The

origin areas are classified as rural-farm, rural-nonfarm, and urban, with additional classification by size of urban place. Each stream of migration is considered separately for joint classifications by sex, age, and color. This elaborate system of cross-classification results in comparisons of a rather precise sort. For example, educational achievement of native-white males of rural-farm origin who migrated to cities may be compared with the educational level of the rural non-migrants of similar control characteristics, within each of eleven age categories. Apart from the control characteristics of age, sex, and color, eight social and economic characteristics are considered for each migration stream.

The authors have done a masterly job in bringing out of this great mass of detailed data a series of meaningful specific findings. These are further condensed in summaries which are integrative rather than merely repetitive condensations. The meaningful interpretations arising out of this mass of data are made possible by the authors' development and use of several simple statistical measures for analyzing and presenting migration differentials. Unfortunately many previous uses of the important 1935-1940 Census migration data have resulted in a morass of trivial and unintegrated findings that are difficult for even the "expert" to wade through. It is a pleasure to have here a pilot demonstration of an orderly analysis that is significant and easy to follow, despite the complexity of the data presented.

We can only illustrate some of the interesting findings emerging from the study:

1. Young rural migrants to cities in the "Corn Belt" were not a depressed group. They succeeded in obtaining jobs more frequently and in about the same occupational pattern as urban non-migrants of about the same age.
2. Migration did not generally alleviate hardship and unemployment in hard-hit areas. There was relatively little out-migration from rural areas of persons whose characteristics indicated economic duress (e.g., long unemployment).
3. In inter-urban migration, there was a selection of better-educated persons, those in white-collar occupations, and those earning relatively good incomes. This is interpreted as indicating that "one major driving force behind internal migration to cities and among cities appears to be the need to distribute and redistribute the potential and actual specialists, experts, and managers to places where their abilities can be used most profitably in the economy."

In a final summary chapter, the authors present "an approach to a theory of differential migration." The nine generalizations presented do not constitute "a theory" in the sense of a logically interrelated set of propositions. They are important but more or less discrete statements or conclusions arising out of the detailed substantive analysis. Examples of these statements are those listed in the previous paragraph.

One of the principal purposes of the study is to make data available and to provide a frame for more detailed studies. In addition to 113 pages of base data contained in an appendix, the authors provide instructions for effective access to a large number of unpublished tabulations. These tabulations involve further cross-classifications of characteristics treated in the main body of the report. In this way the authors have made it possible for individual scholars to participate in the analysis of an important set of data. The expensive and arduous task of bringing the data to this point is frequently beyond the resources—and, sometimes, the competence—of the individual investigator. In their present processed form, the data are particularly suitable for doctoral dissertations. A number of very interesting hypotheses can be investigated, pushing forward explanations suggested in the substantive part of the study.

This is an age in which the masses of data collected may overawe the individual scholar or lie beyond his resources. This report illustrates admirably how government agencies and research organizations may bring data to a stage at which individual research projects are possible and may be of cumulative value.

RONALD FREEDMAN

University of Michigan

The Farmers' Movement, 1620-1920. By CARL C. TAYLOR. New York: American Book Company, 1953. 519 pp. \$5.50.

Back in 1921, John R. Commons published his two-volume work, *The History of Labour in the United States*. Taylor had been interested in the question of the struggle of the American farmers to solve their price and market problems and had formulated, as early as 1914 the hypotheses that they were, by organized effort, struggling to catch up with the price regime. Commons' work led Taylor to expand his original hypothesis and he states it on page 493: "Just as the various and varying struggles of laborers arose out of, and have always evolved about the issues of wages, hours, and working conditions, and just as all these struggles combined constitute the American Labor movement, so the various and varying struggles

of farmers arose out of, and have always evolved about, the issues of price, markets, and credits, and all these struggles combined constitute the American Farmers' Movement."

The study is a sociological analysis of this Farmers' Movement. Opening with a chapter entitled "There is an American Farmers' Movement," that sets forth the hypothesis and plan of the study, it analyzes various episodes in American agricultural history beginning with colonial times and the earliest farmers' revolts and continuing down to the developmental period of farmers' cooperatives in the 1920's. The hypothesis is faithfully applied to each episode studied and the reader therefore gets much more than an historical statement about a farmers' revolt or a farmers' organization—he gets this history interpreted sociologically. The Grange, the Farmers' Alliance, the Populist Revolt, the Farmers' Union, the Equity Societies, and the Non-partisan League are each studied in relation to the price, market, credit and other problems accompanying their activity and, therefore, come alive as problems in human relationships. But as important as is the interpretation of each episode, is the general thread that runs through and ties all of these activities together, showing that there has been a real farmers' movement in our American history as genuine as the labor movement—farmers have continuously been struggling together to solve by one means or another their price and market problems.

The general sociologist will be interested in this volume, for it is a good example of the study of a basic phenomenon in society, namely social movement. This reviewer suggests that reading the preface, chapter one, and chapter twenty, first would be advisable, as these statements present the theoretical framework.

The sociologist emphasizing rural life will be even more interested in the study, not only because of its theoretical significance, but because it furnishes the foundation for practical work in teaching. One of the lacks in the development of Rural Sociology in the United States has been this sociological analysis of our agricultural history. While some may say that the theory is incomplete, it is nevertheless a theory and one can add to or take from it as facts warrant.

More rural sociologists should now begin to offer courses in Farmers' Organizations and Farmers' Movements in the United States. In this regard, this reviewer can affirm that this volume makes an excellent basic text. Several years ago I decided to offer such a course at Cornell. I knew of Taylor's work; in fact, I was a member of his first seminar on this subject at North Carolina State College in the

early twenties. I wrote Taylor and he sent me his typed manuscript. I used it in my seminars and the students appreciated it for its content and readability. Likewise, the volume ought to stimulate further sociological study of past and present day farmers' organizations and activities by rural sociologists.

Taylor has carried the Farmers' Movement in the United States down to 1920 in this work. He proposes to analyze farmers' organizational activities after the twenties in another study. In fact, I know he already has much work done on this analysis. This reviewer takes a little credit for persuading him to publish the present volume before this later task was completed. Here he wishes to urge him to complete this next task as soon as possible.

The volume is put up in attractive style and excellent type. It contains a useful selective bibliography, full notes, and complete index.

W. A. ANDERSON

Cornell University

The Impact of Russian Culture on Soviet Communism. By DINKO TOMASIC. Glencoe, Ill.: The Free Press, 1953. 287 pp. \$4.50.

Soviet culture, that is the culture prevailing today in the area which, until recently, was called Russia, is the result of the blending of the millennial culture tradition of Russia with the socialist ideal in its Marxian form. Its study presents interesting sociological problems since its genesis is quite at variance with the familiar process of culture contact. Commonly, these problems are approached from the angle of view of the impact of the dynamic force of Communism on the relatively stable Russian culture. Mr. Tomasic has chosen to make a study in reverse and to find out what has been the impact of Russian culture on Communism.

His book is divided into five parts, but actually consists of two. The first, occupying the major part of the book, is devoted to the Russian culture. The second, consisting of 54 pages only, concerns itself with Soviet culture in the meaning above. Unfortunately, none of the parts is adequate to the exigencies of the problem.

Russian culture is treated genetically, as the result of the blending of two cultures, that of the "power seeking horsemen of the steppes" and that of the "power in different old-Slavonic plowmen." The former is described as patriarchal and authoritarian, inciting men to mistrust and cruelty; the other is assumed to have been matriarchal and democratic, even anarchic, inciting men to cooperation. The merger of the two cultures is asserted never to have been complete; hence something akin to split person-

ality is considered to have been the dominant trait of the Russian culture.

The approach could have been fruitful if, behind the assertions, there had been fairly reliable historical evidence. But there is none. The ideal type of Turco-Mongol culture is constructed by Mr. Tomasic on the basis of scattered evidence concerning individual peoples of the two groups at different ages, quite in the style of classic evolutionism. The idyllic picture of old Slavonic civilization is constructed in a similar way and is full of contradictions. Thus, for instance, Mr. Tomasic believes that the homeland of the Slavs was in the Pripet marshes; but, on page 41, he quotes, without explanation or criticism, a famous passage from Nestor's Chronicle describing the inhabitants of the marshes as a cruel, bellicose and licentious people. Of course, there is not the slightest historical reason to believe that the old-Slavonic family was matriarchal. Finally, historically, there never was a blending of Turco-Mongol culture (the very existence of which is subject to doubt) and old-Slavonic culture. Prior to the Tartar conquest (middle of the 13th century) the culture of the Eastern Slavs was a blending of proto-Slavonic culture, not very much different from early Teutonic culture, with Byzantine culture; the process and the result have been masterfully presented in G. Vernadsky's *Kievan Russia*. The Tartar conquest, as known to all historians, has only slightly affected Russian culture because, as the author concedes, the Tartars did not care to convert the conquered peoples to their way of life.

The picture of Soviet culture offered by the author is in general correct, but not original; what Soviet society and culture are is no longer a puzzle. But the five chapters devoted to it are poorly organized and are quite helpless when the impact of Russian culture on Communism is mentioned. In a limited number of cases, the author classifies one or another trait of Soviet culture as going back either to the gentle culture of the old Slavs or to the ferocious culture of the Turco-Mongol; more frequently, he points to the recurrence, under the Soviets, of some practices of Tsarist Russia. In all cases, he proceeds in an impressionistic manner; he sees similarity and jumps to the conclusion that Soviet culture may be explained in terms of his genetic hypothesis.

In consequence, the present reviewer is unable to summarize Mr. Tomasic's views on the impact of Russian culture on Communism, for on this question, the author does not offer anything tangible.

N. S. TIMASHEFF

Fordham University

Americans and Chinese, Two Ways of Life. By FRANCIS L. K. HSU. New York: Henry Schuman, Inc., 1953. xi, 457 pp. \$6.00.

We are familiar with the accounts of China by American writers, but have seldom come across any writings by Chinese which interpret life in America for Western readers. In this respect, *Americans and Chinese* is unique in so far as it represents the first serious attempt at a comparative analysis of the two cultures by a Chinese anthropologist. Dr. Hsu, educated in China and Britain, has for years taught and undertaken research studies in both Chinese and American universities. His thorough mastery of both languages, and his profound knowledge of and intimate contact with both cultures give him a rarely qualified position to expose and compare the two ways of life.

The main thesis employed throughout the book is found in the prologue. According to the author, the fundamental difference between the Chinese and American ways of life can be reduced to two sets of contrasts. First, the emphasis of the Chinese culture is upon "an individual's appropriate place and behavior among his fellowmen," a characteristic which is termed *situation-centered*; whereas the stress of American culture is laid on "the predilection of the individual," a characteristic called *individual-centered*. As a corollary, the second contrast is "the prominence of emotions in the American way of life as compared with the tendency of the Chinese to underplay all matters of the heart" (p. 10).

Adhering to this thesis, Dr. Hsu goes on to make comparisons between different facets of life in America and China. Part I is concerned with an examination of art and literature, and also the conduct between the sexes and aberrant behavior. The contrast between the two family environments, the opening attitudes toward child care and rearing, and the different ways of training in schools is emphasized.

Part II is devoted to the comparison of the characteristic attitudes of the individual and the expressions of these attitudes in the major institutions. For instance, the Chinese who are emotionally quite satisfied in the primary groups have little or no desire to bother about politics or business adventure, and the Americans driven by the feelings of insecurity dash out to strive for personal success. The weakness of this section seems, in the reviewer's opinion, to lie in the discussions on religion in which, instead of a contrast between Christian theism and Confucian humanism, the two dominant national faiths, the difference is described in the realm

of monotheism versus polytheism. Polytheism in China is exemplified by Buddhism which was originally an Indian atheism.

Attention in Part III is turned to the area of social problems. The interesting points of contrast are that "Not only are the weaknesses of each people their price of their strength, but the weaknesses of the Chinese tend to occur in the sphere of American strengths and vice versa" (p. 326). As to the current issues, the emergence of Communism in China is ascribed to internal poverty and oppression plus external invasion by Western powers and Japan. Both forces have for a century operated to bring about collapse of the traditional order.

Americans and Chinese has once more proven Dr. Hsu's lucid style, keen observation, and penetrating analysis. If there is any comment to be made, it could be made on the main thesis.

In Dr. Hsu's presentation each of the two ways of life is taken as a unit and each unit is reduced to one or two points in contrast, such as individual-centered versus situation-centered, or self-reliance versus mutual dependence, etc. The question arises as to whether either culture is homogeneous and uniform enough to allow one to draw such generalizations. If similar contrasts could also be made between different segments of the same culture, say, between life in rural and urban areas, Dr. Hsu may not find himself to have dealt with the same phenomenon that appears in a continuum with the folk culture at one end and urbanism at the other, to the exposition of which so many students of human society have concerned themselves?

SHU-CHING LEE

Washington University

BOOK NOTES

California Sexual Deviation Research. Published by California Department of Mental Hygiene and Langley Porter Clinic. California State Printing Office, 1953. 156 pp. No price indicated.

This is the second interim report based on findings of the California survey research initiated in August 1951 on sex crimes and delinquency.

The report, divided into a number of independent sections, each by separate authors, gives in the first section by Dr. A. R. Mangus, general administrator of the studies, a statistical study of sex offenders in California. Much of these data are valuable additions to our knowledge of crime.

The second section, which includes papers by seven different authors, most of them M.D.'s, is devoted almost exclusively to the study of the social and personality factors which select particular children as sexual victims. The study of these victims is largely clinical. It was found that about two-thirds of the 74 victims studied consciously or unconsciously encouraged the offender, and were therefore classified as participant victims. In general, the participant victims were emotionally disturbed due to unsatisfactory parent-child relationships. The child entered the sexual relationship, usually with a person well known to the family, in order to gain affection, attention, and approval and/or as a punishment to relieve a sense of guilt which had its origin in the home life of the child.

Later sections, probably of lesser interest to the sociologist, report on a psychophysiological

study of sex deviates, a review of scientific literature on sexual deviation (including the problem of castration as a treatment), and methods of sexual deviation research.—ROBERT E. CLARK.

The Population of Czechoslovakia. By WALLER WYNNE, JR., Bureau of the Census. Washington: U. S. Government Printing Office, 1953. 72 pp. \$40.

Students of population are prone to speculate on the demographic consequences of political upheavals within a country when adequate statistics are lacking. Czechoslovakia, created in 1918, dismembered in 1938, re-established in 1945, has been one of these areas of speculation.

This report brings together in one compact volume the available demographic data as of December 1, 1952. Compilations include population data on distribution, social characteristics, employment, and population forecasts by age and sex to 1960. Additional data on nationality, religion, and school enrollment are also presented.

While much of the information presented is of dubious validity, this is a function of the unstable nature of the population of Czechoslovakia. Geographically the Czech regions are similar in demographic and cultural characteristics to countries of Northern and Western Europe. The Slovak regions more closely resemble those of Eastern and Southern Europe. Precise information on migration is virtually lacking. The expulsion of some three million Sudeten Germans, thousands of Jews, and

Hungarians has undoubtedly altered the character of the regions considerably. In February, 1948, when the Communist Party came into control, an additional estimated 50,000 persons fled as political refugees, while at the same time scores of ethnic Czechs and Slovaks returned from various countries in Europe and elsewhere.

Infant mortality, frequently used as an index of the health and well-being of a population is still at a relatively high rate in Czechoslovakia. During the period 1942-1946, for every thousand live births about 115 infants died before reaching their first birthday, and by 1950 infant mortality was at the rate of 78.3 per 1000 live births—mute testimony of the ravages of war and the toll it reaps in human life.—HEINZ J. GRAALFS.

Changing Military Patterns of the Great Plains, Locust Valley. By FRANK RAYMOND SECOY. New York: J. J. Augustin Company, 1953. 112 pp. No price indicated.

This monograph of the American Ethnological Society traces the diffusion of guns and horses among Plains Indians over a span of two centuries, from about 1630-1830. The writer made a contribution by reconstructing a few segments of the culture of America's aborigines by expertly piecing together bits of data found in more than one hundred sources, many of them primary historical records. These sources were poor material for getting a rounded picture of Indian cultures and culture change. They were written down largely by traders, soldiers and missionaries. Their accounts of Indian cultures are strongly colored by their ethnocentricity. Most of them regarded the Indians as "savage," "hostile," "primitive," or "unChristian." The study is more historical than anthropological. It illustrates the difficulty of getting a coherent historical account of people who could not write or otherwise preserve their story for posterity.—JOSEPH W. EATON.

Menomini Peyotism: A Study of Individual Variation in a Primary Group with a Homogeneous Culture. By J. S. SLOTKIN. Philadelphia: Transactions of the American Philosophical Society, New Series Volume 42, Part 4, 1952. pp. 565-699. \$2.00.

This monograph deals with the Native American Church as it is found among Menomini Indians living in Wisconsin. The distinguishing feature of the Native American Church is the use of the drug, peyote, to induce what are deemed by participants to be appropriate religious states. The author's general reflections on peyotism are confined to the first few pages; the next 125 pages are devoted to long quota-

tions, verbatim interviews, and musical transcriptions, held together by brief connecting statements. The major intent of the monograph is indicated by the subtitle. In so far as it is a study of a primary group it should be of interest to sociologists as well as to anthropologists.

But is this monograph really a study of a primary group with a homogeneous culture? Only in the most casual of senses. There is nothing on the culture of the Menomini as a submerged or "acculturated" minority and next to nothing on the primary group which is supposed to possess this culture. There is no analysis of the social organization of the participants in the peyote sect or even of the kinship relations—a particularly puzzling neglect considering that the author is an anthropologist. The argument that this is a study of a proto-theology and that the more general works on peyotism and on the Menomini are cited in footnotes is a weak defense for such omissions, especially in the light of the subtitle. There appears to be a rough and general kind of agreement among participants as to beliefs and practices but the author ventures few comments on this; he is more interested in letting the facts, and the Menomini, speak for themselves. The former never do because they never can; the latter do so but haltingly, their lack of facility in verbalization and/or abstraction adding to the impression of individual variability. Since the author makes no attempt to analyze the kind and extent of variability in crucial dimensions we have no way of assessing even this potentially significant material.—MICHAEL S. OLMSTED.

Social Theorists. By CLEMENT S. MIHANOVICH (Ed.). Milwaukee: The Bruce Publishing Company, 1953. 521 pp. \$6.50.

The social theories summarized and discussed in the text are intended primarily as introductions to the "schools" and trends in social theory. An appendix of "Who's Who in Social Theory" is designed to supplement the incomplete coverage of different phases of the history of sociology. The lack of agreement on subject matter, methods, and objectives of sociology (by the authors of various chapters) and the failure to distinguish between theology, social philosophy, social action, and social science make criticisms of the theories presented less noteworthy than they might have been.

The lack of basic agreement in sociology is the subject of the last chapter, "The Future of Sociological Theory," by Gerald J. Schnepf. Current sociological theories and some of the general problems, including the problem of definition of sociological theory, are outlined

with reference to the main positions of modern sociologists. Although many sociologists will not agree that a "universally acceptable synthesis" of sociological theories is either possible or desirable, in view of the history of other sciences, all sociologists probably will approve the author's desire for an unbiased evaluation of theories. Unfortunately, the criteria for objective judgment in sociology also are the subject of much disagreement.

One of the positive contributions to sociological theory which might be expected to further agreement, the realistic approach to the study of society, receives little attention either from the historical or the critical viewpoint. Bibliographical references not common in other textbooks on sociological theory, however, should stimulate further investigation of the possibilities in a realistic orientation.—JARVIS M. FINLEY.

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